JOURNAL of THE IRON AND STEEL INSTITUTE

VOL. 157 PART 1

SEPTEMBER 1947

Editor . . K. HEADLAM-MORLEY
Technical Editor . . A. E. CHATTIN, B.Sc. (Hons. Met.) A.R.I.C., F.I.M.
Managing Editor . E. R. MORT, B.Sc., F.I.M.
Assistant Editor . . C. R. HARMAN, B.Sc. (Eng.)

PUBLISHED MONTHLY

Editorial contents copyright by THE IRON AND STEEL INSTITUTE

EDITORIAL AND ADVERTISEMENT OFFICES

4, Grosvenor Gardens, London, S.W.1

Telephone : SLOane 0061

Telegrams : Irosamente, Sowest, London

SUBSCRIPTIONS

Members : Free
Extra Copies 7s. 6d. each

Non-Members : £5 . 0 . 0 per annum
Single Copies 10s. each

NOTE : The Institute as a body is not responsible for statements made, or opinions expressed, in papers, articles or discussion appearing in the Journal of the Iron and Steel Institute.

CONTENTS

IRON AND STEEL INSTITUTE PAPERS

"The Fluidity of Steel" *by R. Jackson, D. Knowles, T. H. Middleham and R. J. Sarjant* .. 1
"Anglo-Saxon Cutlery" *by H. R. Schubert* .. 22
"Behaviour of a Chromium Steel in the Jominy Hardenability Test" *by W. I. Pumphrey* .. 27
"Deformation of Metals during Single and Repeated Tensile Impact" *by J. A. Pope* .. 31

DISCUSSION ON PAPERS

Spring Meeting, 1947
Symposium on the Hardenability of Steel. Special Report No. 36 .. 55

BRITISH IRON AND STEEL RESEARCH ASSOCIATION

"Installation and Use of Instruments on Open-Hearth Melting Furnaces" *by R. C. Baker* .. 81
"A Micro-Examination of Eight Steels for the Inclusions Sub-Committee" *by J. H. Whiteley* .. 89
Combined Meeting on Converter Practice—General Discussion
First Report of the Converter Refractories Sub-Committee .. 99
First Report of the Side-Blown Converter Practice Sub-Committee .. 104

IRON AND STEELWORKS ENGINEERING

Report of Fourth Meeting (Afternoon Session)—General Discussion on "Gas Turbine Applications in Iron and Steel Works" *by A. T. Bowden, W. H. Gibson, J. W. Railly and R. G. Voysey* .. 115

NEWS

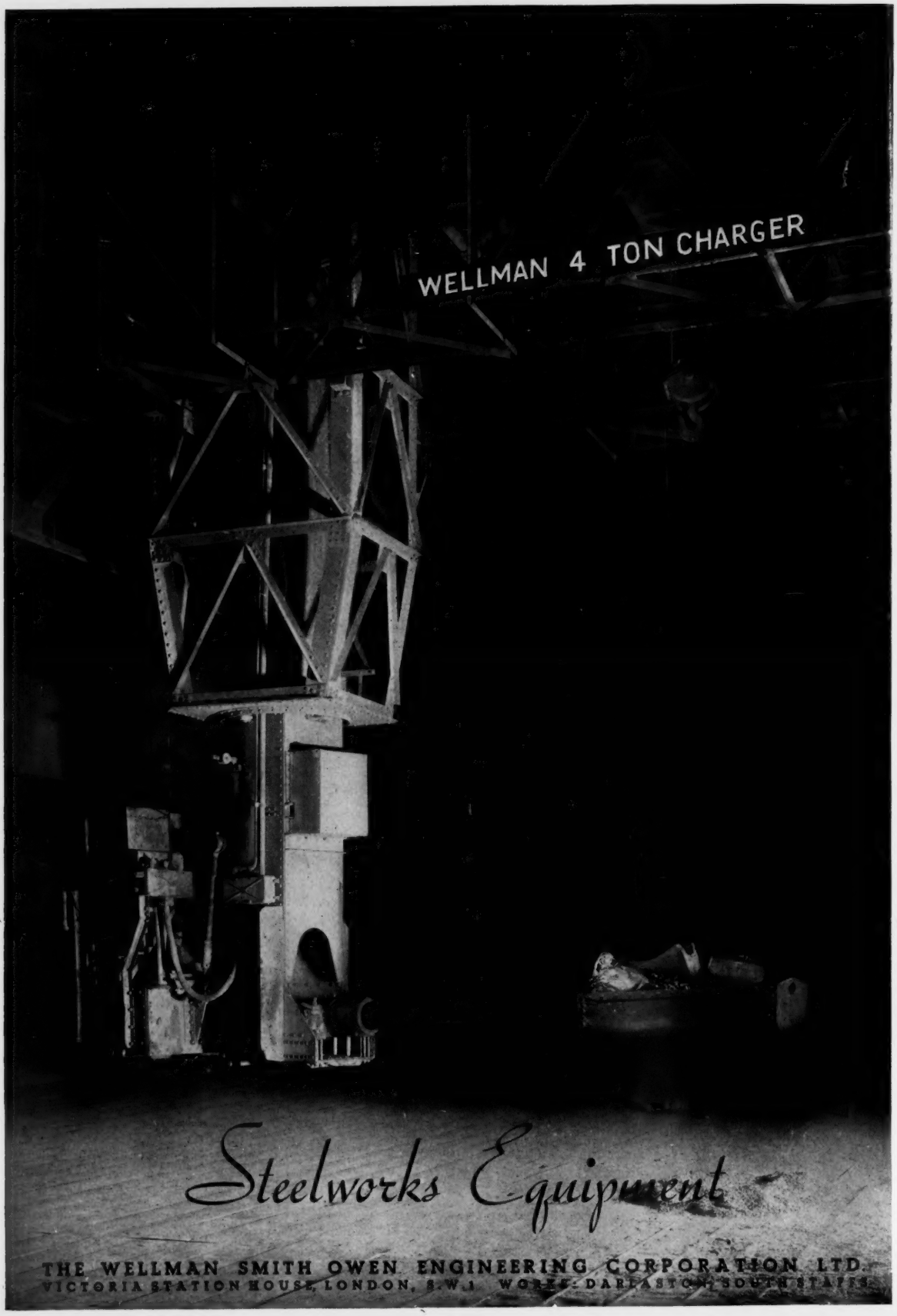
The Iron and Steel Institute .. 131
News of Members .. 132
Iron and Steel Engineers Group .. 135
British Iron and Steel Research Association .. 135
The Institute of Metals .. 135
News of Science and Industry .. 135
Translation Service .. 138

ABSTRACTS and BOOK NOTICES

Contents .. 139
Abstracts .. 139-158
Book Notices .. 158
New Publications .. 160

MAY 24 1949

XUM


WELLMAN 4 TON CHARGER
Steelworks Equipment
THE WELLMAN SMITH OWEN ENGINEERING CORPORATION LTD.
VICTORIA STATION HOUSE, LONDON, S.W.1 WORKS: DARLASTON, SOUTH STAFFS.

XUM

PROFESSOR J. H. ANDREW, D.Sc., F.I.M.

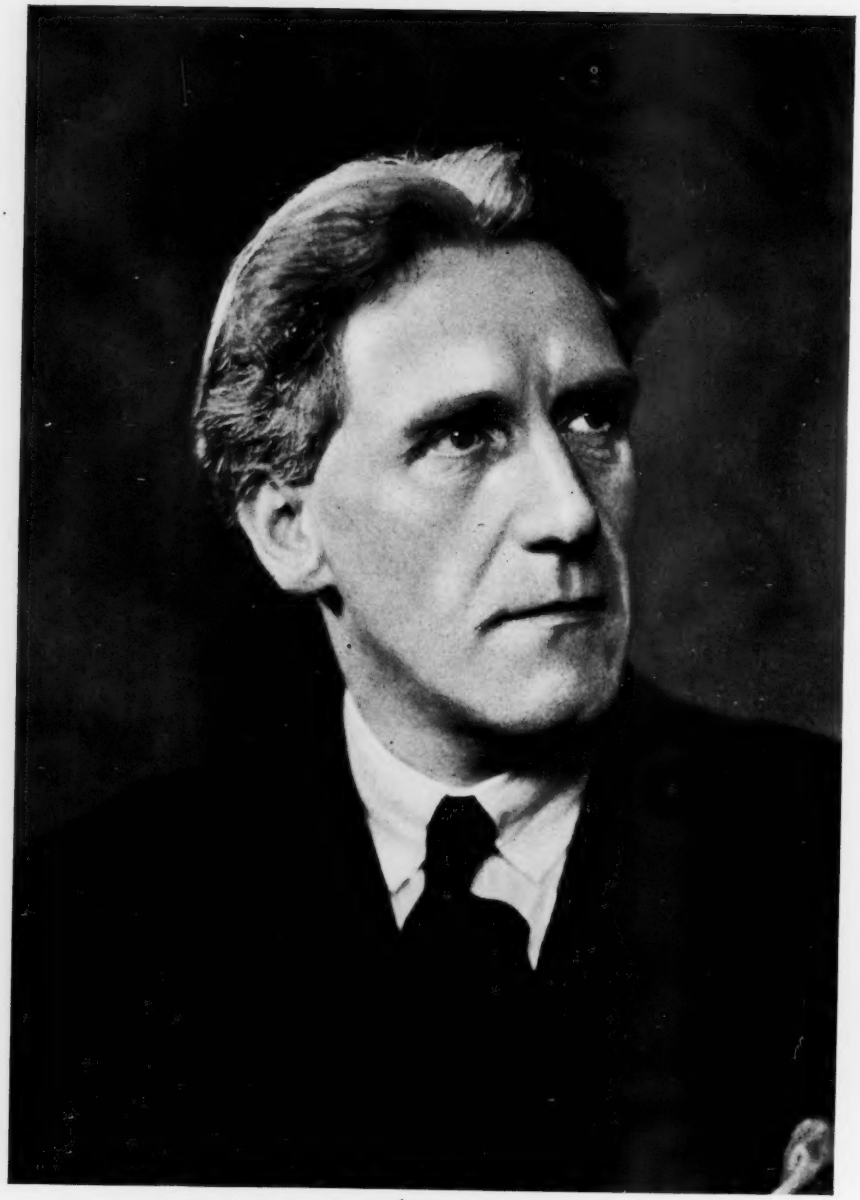
PROFESSOR J. H. ANDREW, D.Sc., F.I.M., was born on 14th January, 1887. Educated at Bickerton House School, Birkdale, he received his scientific education at Manchester University, where he took a First Class Honours degree in Chemistry in 1907 ; later he gained the M.Sc. degree, and was awarded the degree of D.Sc. in 1915. In 1910 he was appointed a Research Fellow and Demonstrator in that University, of which he was a Dalton Scholar.

In 1914 Professor Andrew became Head of the Metallurgical Research Department of Sir W. G. Armstrong, Whitworth & Co., Ltd., of Manchester, where he was fortunate to have as his immediate chief Mr. E. F. Law, and he retained this position until 1920. In that year he was appointed Professor of Metallurgy at the Royal Technical College, Glasgow, succeeding Dr. C. H. Desch, F.R.S., who had been appointed Professor of Metallurgy at Sheffield University. Professor Andrew remained at the Royal Technical College, Glasgow, until 1932, when he again succeeded Dr. Desch, this time as Professor of Metallurgy and Dean of the Faculty of Metallurgy at Sheffield University, a position which he still holds.

During the past thirty years, Professor Andrew has gained for himself a prominent position in the metallurgical world, particularly in regard to research ; a large number of investigations have been carried out in the Metallurgical Department of Sheffield University under his direction. He was a prominent member of the Committee on the Heterogeneity of Steel Ingots, the Alloy Steels Research Committee and the Steel Castings Research Committee—three of the former Joint Committees of the Iron and Steel Institute and the British Iron and Steel Federation reporting to the Iron and Steel Industrial Research Council (all of which have been absorbed into the recently constituted British Iron and Steel Research Association)—and much investigational work for these Committees was carried out under his care in the University. He is now Chairman of the Steel Castings Panel of the British Iron and Steel Research Association, and is also one of the representatives of the Iron and Steel Institute on the Council of that Association.

Professor Andrew has made many notable contributions to the literature of metallurgy. He has been responsible either as author or co-author for a large number of papers, of which many have been presented to the Iron and Steel Institute.

Professor Andrew was elected a Member of the Iron and Steel Institute in 1911 and became a Member of Council in 1941. He is a Founder Fellow of the Institution of Metallurgists, of which he was one of the original Vice-Presidents. He is an Original Member of the Institute of Metals, and was a Vice-President for the years 1938-1941 ; he was re-elected a Member of Council for the period 1944-1947. Professor Andrew is a member of the Executive Committee of the National Physical Laboratory, and one of the Iron and Steel Institute representatives on the General Board of that Laboratory. He is also a member of the Institute of British Foundrymen.



Professor J. H. Andrew, D.Sc., F.I.M.

Member of Council

IRON AND STEEL
INSTITUTE PAPERS

PLANT DESIGN · MANUFACTURE · RESEARCH · DEVELOPMENT

The Fluidity of Steel*

By R. Jackson, Ph.D., F.I.M.,† D. Knowles, A.Met., F.I.M.,†
T. H. Middleham† and R. J. Sarjant, O.B.E., D.Sc.†

SYNOPSIS

The paper describes experimental investigations on the fluidity/temperature relationships, as indicated by Ruff and Spiral mould tests, of four steels, viz., *(1) 2% Cu steel, (2) Si–Ni steel, (3) low-carbon steel, and (4) 13% Mn steel, melted in high-frequency furnaces with acid and basic linings.*

Temperature measurements were made by optical and quick-immersion-couple methods. The Spiral mould gave more consistent results than the Ruff.

Comparison of the authors' results with those of Taylor, Rominski and Briggs on similar steels showed widely differing fluidity/temperature relationships with similar moulds. Investigation of the pyrometric methods employed in the two sets of trials indicated that the differences could be mainly ascribed to the time lag in the temperature measurement employed by the American authors.

THE present work was undertaken primarily to compare the results of work published by Taylor, Rominski and Briggs[1] with that[2] of two of the present authors (R.J.S. and T.H.M.) on the relationship between the fluidity of molten steel and temperature. Our earlier work[2] had been confined to an examination of the fluidity/temperature relationship in a small 18-lb. high-frequency furnace, from which steel was poured directly into a mould of the Ruff type. The American investigators[1] had used a spiral type of fluidity-test mould in preference to one of the Ruff type. Incidentally, in the discussion of the paper by Kron and Lorig[3] it had been argued by Briggs that the form of curve published in our previous paper was impossible, and that the curve should tend to become parallel to the temperature axis and not to the fluidity one. That the fluidity, as indicated by those earlier experiments, apparently tended to change at high temperature in a direction opposite to that which would be expected on *a priori* grounds, had been realized by the authors at the time of carrying out the experiments. For this reason the observations had been carefully checked and repeated to confirm the experimental indications. A number of the steels used by Taylor, Rominski and Briggs had been of a very fluid type and differed from those which Sarjant and Middleham had used. In order to check whether any agreement would be reached on repeating fluidity tests using a spiral mould similar to that employed by the American authors, and with a steel which was of similar composition to one of their steels, a preliminary series of trials was carried out by the present authors on the same 18-lb. high-frequency furnace as that which had previously been used. The results of these preliminary experiments are shown in Fig. 1, from which it was apparent that there were major differences in fluidity/temperature relationships. Comparison between the results obtained from the Ruff and the Spiral moulds showed that the shape of the curve in these two cases was the same, and confirmed that obtained with the Ruff mould in the earlier experiments. The fluidity as determined by the Spiral mould was of a higher order, for the same temperature, than that shown by the Ruff mould, but a consistent relationship was apparent with variation of temperature.

* Received 31st January, 1947.
† Messrs. Hadfields, Ltd.

XUM

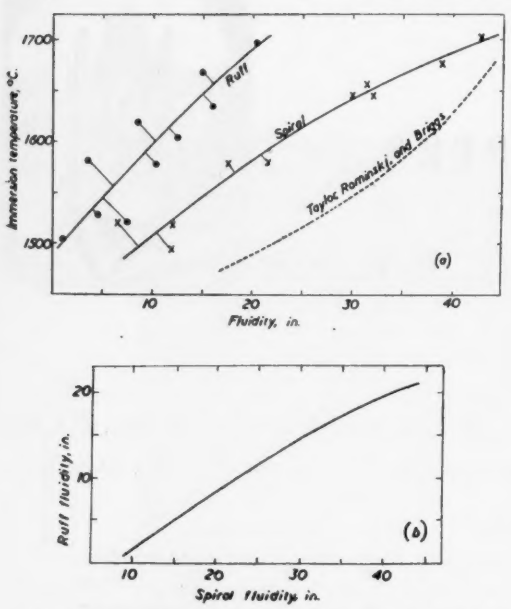


FIG. 1—Fluidity tests. (*a*) Comparison of Ruff and U.S. Naval Research Laboratory Spiral moulds. Tests on Si–Cu steel melted in 18-lb. high-frequency furnace. (*b*) Curve showing Ruff/Spiral fluidity relationships

Further, there was still a major discrepancy in the temperature/fluidity relationship as obtained in these new experiments on the Spiral mould, when compared with those reported by Taylor, Rominski and Briggs.*

It was realized that with the small furnace a curve had to be determined by casting the test pieces from many individual melts, with the possibility of minor variations in composition of the steel. In order to approach more nearly the conditions obtained in the American experiments, further trials were made in a larger high-frequency furnace, namely, of 2 cwt. capacity, from which it was possible to establish a temperature/fluidity curve from one heat.

In order to compare the effect of the change in the size of the melting furnace, a second series of experiments was carried out using a steel of similar composition to that used in the smaller furnace. Our earlier results were again confirmed, as shown in Fig. 2, except that with the larger furnace the shape of the fluidity/temperature curve had more concordance with that of the American workers, but there was still a wide difference in the position of the curves obtained

* For brevity these authors will be referred to as Taylor and others.

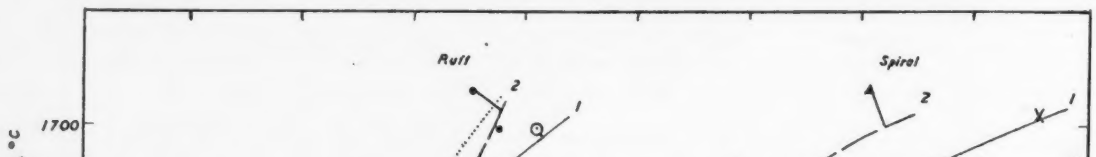


FIG. 2—Fluidity tests. Comparison of Ruff and U.S. Naval Research Laboratory Spiral moulds. Tests on Si–Cu steel. (1) Melted in 18-lb. high-frequency furnace. (2) Melted in 2-cwt. high-frequency furnace. (3) Curve of Taylor and others

with the Spiral mould. These results prompted the more detailed series of investigations described in the sequel.

Main Features of the Method Employed

A 2-cwt. high-frequency furnace was used to melt the steel which was poured into a 70-lb. hand-shank and cast from this into the moulds. At appropriate intervals four sets of moulds were cast from this shank, which was then refilled. Each set consisted of a Ruff and a Spiral mould, cast in quick succession. It was usually possible to obtain 12 sets from each heat, which were sufficient to define a curve of the relationship between the various quantities measured. Temperatures were determined by an immersion couple held in the shank, before and after casting ; the temperature at casting being obtained by interpolation to the time of the actual pouring. Optical temperature readings were also taken on the streams.

Four steels were used, corresponding approximately to the four studied by Taylor and others,[1] and the effect of acid and of basic linings in the furnace was investigated.

Comparison of the results obtained with those given by Taylor and others[1] led to further tests of the technique employed, and the work has therefore been divided into two sections, *viz.* :

(i) Comparison of fluidity and emissivity,
(ii) Comparison of temperature-measuring techniques.

Although a large number of steels have not yet been studied, it is felt, in view of the controversy which has already arisen, that the results obtained at the present stage are of sufficient interest to warrant publication, since they might now lead to valuable suggestions for future attacks on the problem.

Section I—Comparison of Fluidity and Emissivity

Method of Testing

The general method of testing is given below ; in certain heats the details varied slightly and mention is made of this in discussing the individual results.

The steel was melted in a suitably lined, 2-cwt. high-frequency furnace, and the temperature was taken when the heat was considered to be ready. It was found to be impracticable to cast directly from the furnace into the moulds, so an intermediate hand-shank was used. The shank, having been preheated over a gas flame, was further preheated by filling it with the molten steel which was held for 15–30 sec. and poured back into the furnace for a short final heating.

The shank, after filling, was moved into a position ready to pour into the first moulds, and an immersion-couple reading was taken as quickly as possible. The first Ruff and Spiral moulds were then poured, the pouring time being of the order of 2 sec. each. Immediately after pouring, another immersion-couple reading was taken. This procedure was continued for four sets of tests, when the shank was refilled, and four more sets of tests poured ; a final filling, and the pouring of four sets of tests completed the trial.

The immersion couple was of the usual quick-immersion type (*see* Section II for details), and readings were taken on a Tinsley potentiometer. A number of couples were used, change-over being effected in the initial experiments by a multiple double-pole switch, and later by using a single lead with suitable plug attachment for connecting to each couple.

The time of all the operations was taken by means of a number of clocks which were synchronized before and after the trial.

Optical-pyrometer readings were taken on the stream of metal whilst pouring, by an experienced operator using a Leeds and Northrup disappearing-filament type of instrument. The readings taken on the streams during the short time of casting of the tests (approximately 2 sec. per test), were facilitated by the fact that two tests were poured in succession. The first enabled a rough setting of the instrument to be made, which was finally adjusted on the second stream. The optical temperatures are therefore only applicable strictly to the casting of the Spiral tests, though their variation from those for the casting of the first test, the Ruff, would be small, approximately constant, and probably less than 5° C.

Chemical analyses were made of the first and last Ruff tests to be cast, and usually showed the composition of the heat to have remained constant.

By taking the immersion temperatures and their times, it was possible to interpolate to the temperature of the steel at the instant of pouring. Where there is doubt about this temperature, due to a non-uniform drop in temperature with time, the estimated range of temperature within which the value should lie has been given, and is shown on the graphs as a short straight line instead of a single point.

The usual procedure was to use the first shank for getting temperatures in the top range as quickly as possible, the second shank enabled gaps in this range to be filled, and the third shank gave temperatures in the lower range extending almost to solidification temperature.

The shanks were lined with chamotte or a similar neutral lining.

The Spiral test was similar to that used by

XUM

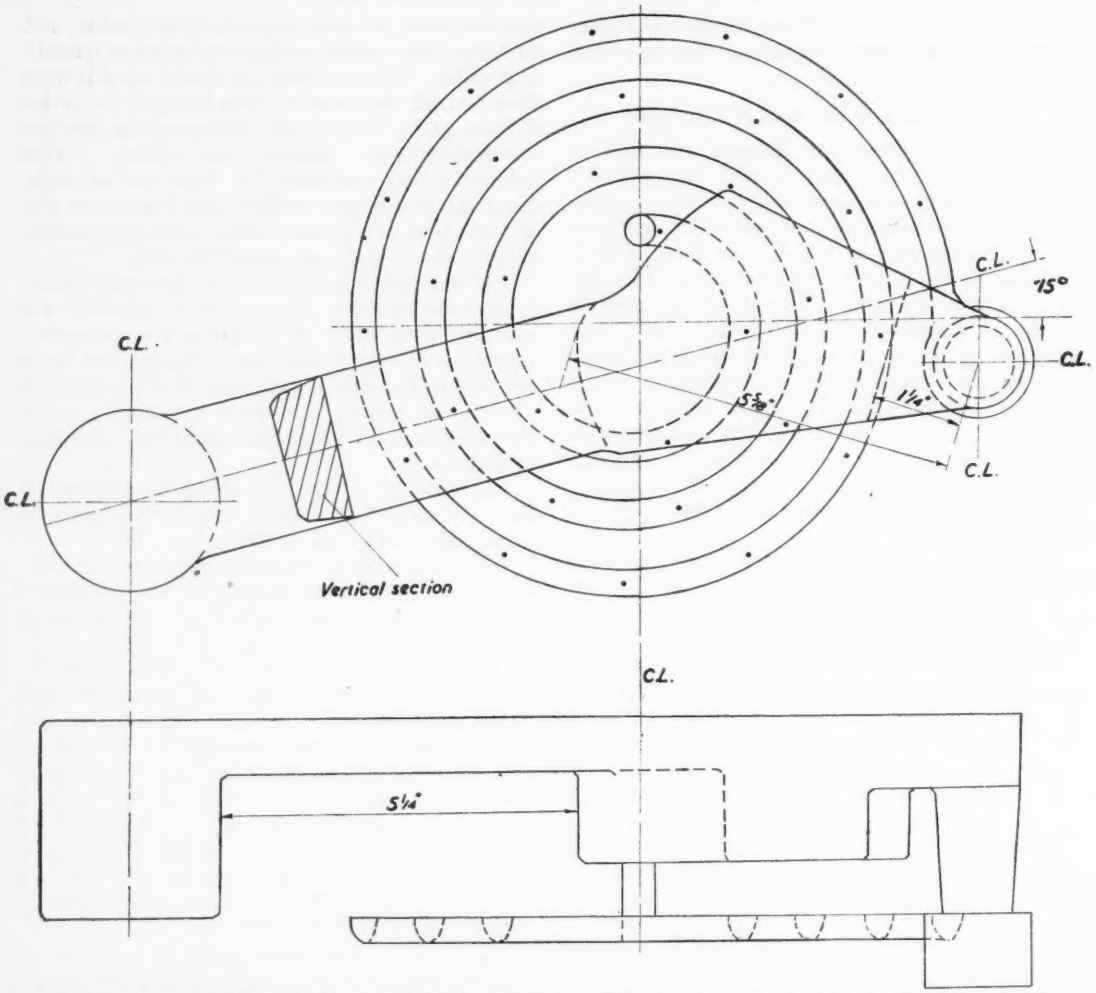


FIG. 3—Details of Spiral mould used. Only the dimensions differing from those given by Taylor and others are included

Taylor and others.[1] The sketch of the mould, given in Fig. 3, shows only those dimensions in which the pattern differed from that quoted by them. The moulds were of dried oil sand. In the later tests the moulds did not close satisfactorily, and several Spiral tests were useless because of fash which connected the separate turns of the spiral.

The Ruff test was made in similar dried oil sand and was of the pattern used by Sarjant and Middleham.[2]

TYPES OF STEEL USED

Four types of steel were investigated which were roughly comparable with the following steels quoted in terms of the heat Nos. and Figure Nos. used by Taylor and others[1] in their paper, namely :

(1) 2% Copper steel—Heat 164, Fig. 56.
(2) Si–Ni-steel—Heat 169, Fig. 56.
(3) A low-carbon steel—Heat 176, Fig. 41.
(4) 13% Manganese steel—Heat 123, Fig. 48.

In the case of steels 1, 2 and 3, heats were made in both acid linings and in basic linings. For steel 4 only a basic lining was used because of the difficulties of melting the steel in an acid lining.

METHOD OF CONSIDERING RESULTS

The chemical composition of the steels, together with details of the type of lining and heat No., are given in Table I. Each heat usually had some feature of its own which was peculiar, and this is mentioned when considering the results. It is convenient in the first place to discuss separately the observations made on each steel.

STEEL NO. 1 (2% COPPER)

Three heats, two from a basic-lined furnace

TABLE I—*Chemical Analysis of Steels Used*

Heat	Steel No.	Type of Lining	Test No.	Chemical Analysis, %									
				C	Si	S	P	Mn	Cr	Ni	V	Mo	Cu
F.295	1	Basic	1	0·22	1·05	0·032	0·028	1·31	0·27	0·12	...	...	1·84
			9	0·18	0·99	0·029	0·026	1·29	0·28	0·12	...	...	1·86
F.302	1	Basic	1	0·20	1·13	0·033	0·017	1·29	0·38	0·74	...	...	1·81
			10	0·21	1·11	0·031	0·023	1·26	0·40	0·78	...	...	1·82
F.432	1	Acid	1	0·19	1·27	0·031	0·019	1·02	0·25	0·06	...	...	1·83
			12	...	1·27	...	...	0·98	...	...	...	...	...
Taylor, Rominski and Briggs' Comparison Analysis				0·21	1·19	...	...	1·08	...	...	...	...	1·74
F.312	2	Basic	1	0·13	1·22	0·021	0·014	1·09	0·17	3·49	...	...	0·07
			12	0·13	1·20	0·019	0·016	1·08	0·19	3·52	...	...	0·07
F.444	2	Acid	1	0·18	1·29	0·024	0·025	0·87	0·20	3·54	...	...	0·07
			12	...	1·29	...	...	0·85	...	...	...	...	...
F.858	2	Basic	1	0·20	1·28	0·027	0·019	1·10	0·52	4·20	...	...	0·08
			12	0·19	1·24	0·027	0·021	1·10	0·55	4·25	...	...	0·08
F.884	2	Basic	1	0·23	1·30	0·033	0·020	1·07	0·50	4·17	...	...	0·15
			12	0·22	1·32	0·033	0·019	1·14	0·54	4·15	...	...	0·16
Taylor, Rominski and Briggs' Comparison Analysis				0·18	1·29	...	...	0·92	...	3·49	...	...	...
F.319	3	Basic	1	0·28	0·07	0·025	0·026	0·61	0·32	...	...	...	0·10
			13	0·26	0·06	0·026	...	0·61	...	...	...	...	...
F.333	3	Basic	1	0·27	0·18	0·031	0·025	0·84	...	...	...	...	...
			12	...	0·06	...	...	0·64	...	...	...	...	...
F.900	3	Acid	1	0·31	0·24	0·040	0·016	0·46	0·12	0·17	Nil	Nil	...
			12	0·29	0·24	0·040	0·016	0·36	0·10	0·17	Nil	Nil	...
Taylor, Rominski and Briggs' Comparison Analysis				0·30	0·41	...	...	0·75	...	...	...	...	...
F.324	4	Basic	1	1·20	1·30	0·012	0·089	12·25	...	...	...	...	...
			12	...	0·94	...	...	12·40	...	...	...	...	...
Taylor, Rominski and Briggs' Comparison Analysis				1·29	0·45	...	...	13·6	...	...	...	...	...

and one from an acid lining were made, the particular features of the heats being given below.

Heat F. 295 (Basic)

This heat was the first made and was not very satisfactory, as the available thermocouples were used at a greater rate than that at which the tips could be replaced. As a result a halt was made in the middle of the heat while this reconditioning was being carried out. A large shank of 1 cwt. capacity was used which proved difficult to handle.

Heat F. 302 (Basic) and Heat F. 432 (Acid)

These heats were satisfactory using a shank of approximately 70 lb. capacity.

Comparison of Immersion and Optical Temperatures

Figure 4 shows the relation between the interpolated immersion temperature for each casting, and the corresponding optical measurement. The three heats are shown separately (as A, B and C) in Fig. 4, and the mean curves for each heat are shown (as D) in Fig. 4. These mean curves are obtained by adjusting the smooth curve so that

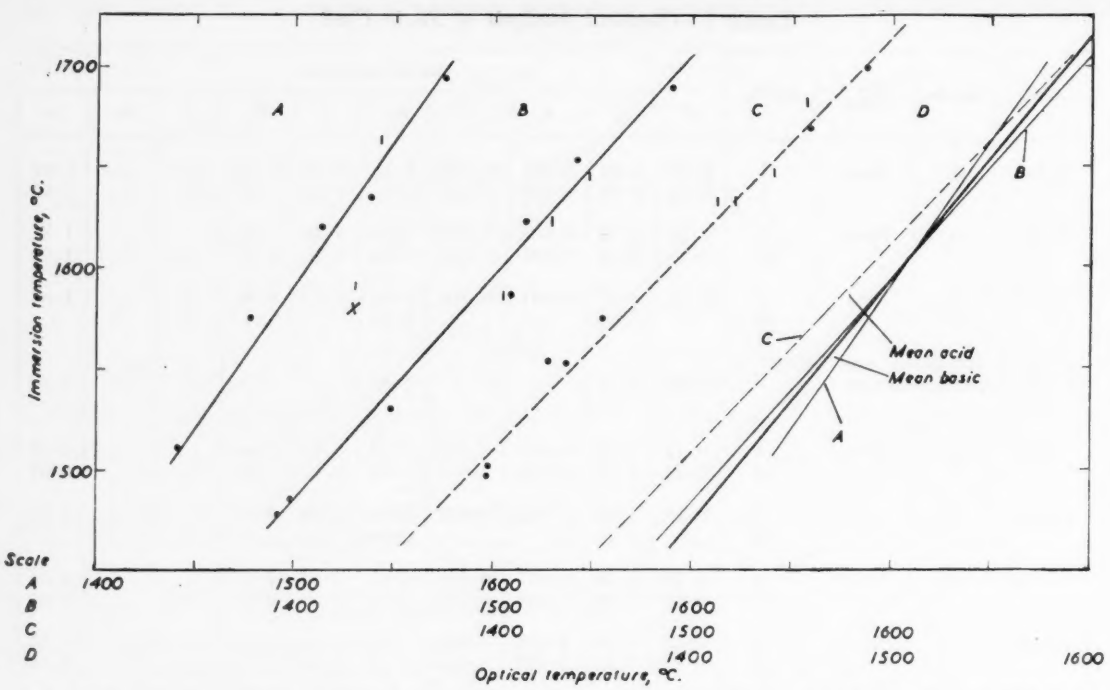


FIG. 4—Immersion temperature *versus* optical temperature. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 1 : (A) Heat F.295—basic ; (B) heat F.302—basic ; (C) heat F.432—acid ; (D) mean acid and basic curves

TABLE II—*Standard Deviations from Mean Curves*

The subscript numbers indicate the number of points wide of the curve and omitted in this calculation

Heat	Steel No.	Furnace Lining	Standard Deviations			
			I/O	I/S	I/R	O/S
F.295	1	Basic	9·6°$_1$	1·15 in.$_2$	2·6 in.	1·7 in.$_2$
F.302	1	Basic				
F.432	1	Acid	8·5°	0·9 in.$_2$	2·2 in.	1·4 in.$_2$
F.312	2	Basic	17·2°	2·0 in.	…	2·5 in.
F.858	2	Basic		0·8 in. } 2·4 in.	…	0·9 in. } 2·5 in.
F.884	2	Basic		…		
F.444	2	Acid	10·5°	2·5 in.	…	2·6 in.
F.319	3	Basic	6·3°$_1$ } 11·3°	0·9 in. } 1·6 in.*	…	1·8 in.
F.333	3	Basic	4·9°$_2$	1·25 in.	…	1·28 in.
F.900	3	Acid	3·1°$_3$	…	…	…
F.324	4	Basic	11·7°	1·2 in.	…	2·5 in.

* Only points above the ' kink ' are considered

Abbreviations :
I/O—Immersion temperature *versus* optical temperature
I/S—Immersion temperature *versus* fluidity by Spiral test
I/R—Immersion temperature *versus* fluidity by Ruff test
O/S—Optical temperature *versus* fluidity by Spiral test

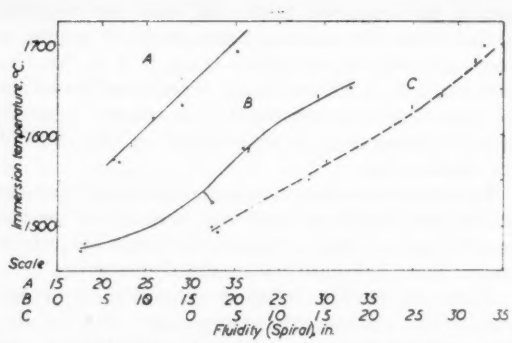


FIG. 5—Immersion temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 1 : (A) Heat F.295—basic ; (B) heat F.302—basic ; (C) heat F.432—acid

the sum of the squares of the deviations is a minimum. As errors are known to be present in both variables, this mean curve is considered justifiable. The lower scale is displaced laterally in each case to separate the curves. The two basic heats give curves lying very closely together, and the mean curve for all the results of the basic heats is also given as a full line in the Figure. Omitting one point shown at X, which is clearly exceptional and is 32° C. away from the curve, the maximum optical-temperature deviation of points from the mean curves is 18° for the acid heat and 14° for the basic heats. The corresponding standard deviation of the points is 8·5° for the acid, and 9·6° for the basic heats. The values of the standard deviations are set out for all the heats in Table II. These values were obtained by assuming that the optical readings alone were in error. As stated in the previous paragraph, errors are also probable in the immersion-temperature readings, so that the actual errors in optical readings may be expected to be less than those given in Table II.

Relation between Immersion Temperature and Fluidity

Curves A, B and C, in Fig. 5, give the relationship between the interpolated immersion temperature, and the fluidity for the three heats separately. Curve B, in Fig. 5 (heat F.302), shows a

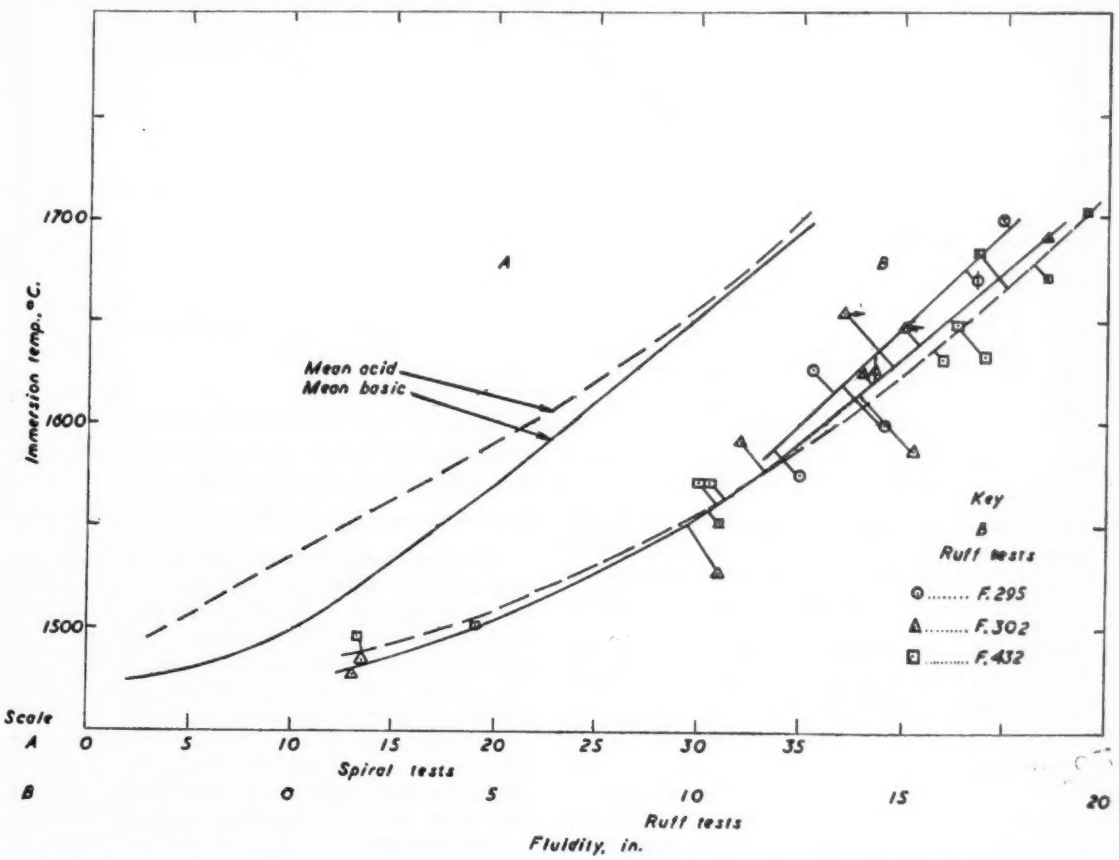


FIG. 6—Immersion temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 1 : (A) Spiral tests, mean acid and basic curves ; (B) Ruff tests

XUM

distinct **S** shape unless the errors in the readings are assumed to be such that a linear relation is more reasonable. Because of the disparity between the shapes of the two basic-heat curves, this possibility has been considered, and the mean curves for basic and acid heats are given (by A) in Fig. 6. The maximum deviation for the length of the Spiral fluidity tests, found from the curves, is 4 in. for the acid heats and 3 in. for the basic heats, or ignoring three values which are clearly exceptional, the maximum deviations are 1·4 in. for the acid, and 2 in. for the basic. The corresponding standard deviations are 0·9 in. and 1·15 in.

For comparison, the corresponding values for the Ruff tests are given (by B) in Fig. 6, the three curves for the two types of test are roughly similar, but the standard deviation of points is much greater for the Ruff, being 2·2 in. for the acid and 2·6 in. for the basic. The linear deviation for the Ruff test represents, of course, a much greater percentage of error than in the case of the Spiral test.

Taking the standard deviation as a percentage of the test length at 1600° C., the values for the two types of test are given in Table III which shows that the Spiral test is clearly superior.

Curve A, in Fig. 6, shows that for this steel, except at extremes of temperature, the fluidity of steel melted in a basic-lined furnace is, surprisingly, greater at a given true temperature than that melted in an acid-lined furnace.

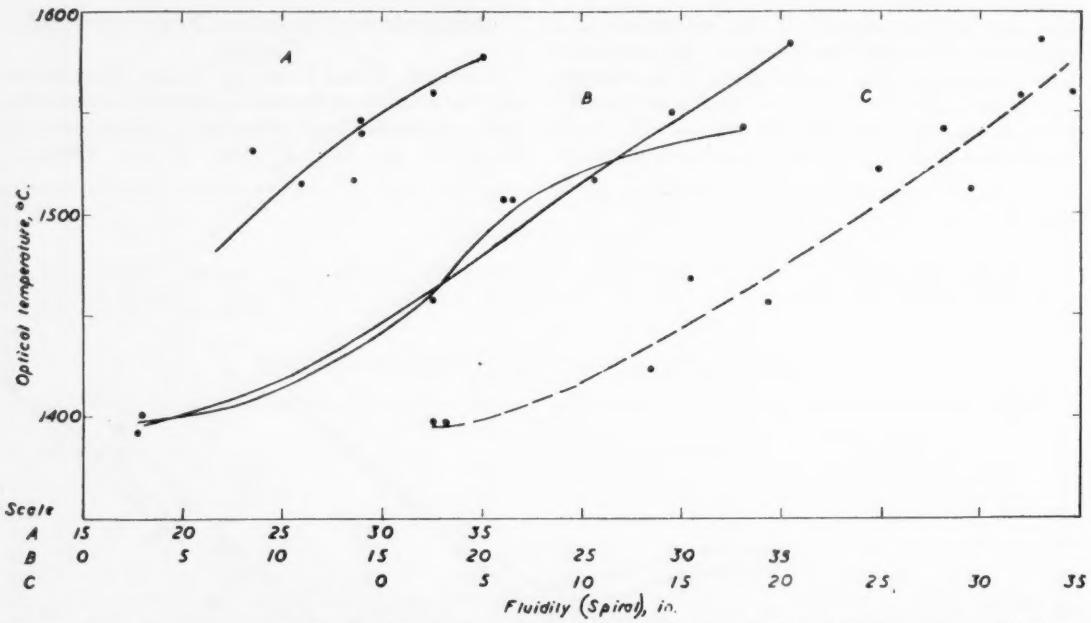


FIG. 7—Optical temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 1 : (A) Heat F.295—basic ; (B) heat F.302—basic ; (C) heat F.432—acid

heats being indicated by separate symbols for the individual points. Because of the difference in length of the Spiral and Ruff tests, the scale of the latter has been doubled. The shapes of the

TABLE III—*Comparison of Scatter in Ruff and Spiral Tests as a Percentage of the Test Length at 1600° C. (Steel No. 1)*

Test	Standard Deviation, %	
	Acid Lining	Basic Lining
Ruff	16	19·3
Spiral	4·3	4·9

Relation between Optical Temperature and Fluidity

The relationship between the optical temperature and the fluidity as determined by the Spiral test is shown (as A, B and C) in Fig. 7 for each heat separately. The **S** shape of the curve B in Fig. 7 would still appear to be justified by the points, but in view of the rest of the results, a simpler line giving a greater degree of error to the points is also shown. This alternative line is the one given (by A) in Fig. 8, which illustrates the curves for all three heats. Similar curves for the Ruff test are given (by B) in Fig. 8.

The standard deviation of the test length, in the curves relating fluidity with optical temperature, is greater for the Spiral test than that in the

previous case, in which fluidity was related to immersion temperature, *viz.*, basic heats, 1·7 in. against 1·15 in.; acid heat, 1·4 in. against 0·9 in. The scatter of the Ruff tests is again obviously greater than that of the Spiral tests.

When optical temperatures are considered, the acid heat would appear to be slightly more fluid than the basic when based on Spiral tests, but much more fluid when based on Ruff tests. By comparison with the previous consideration of the influence of the immersion temperature, this indicates that the acid steel is slightly the more fluid at what seems visually to be the same temperature, but that this greater fluidity is due to an actual greater true temperature which is not visually appreciated.

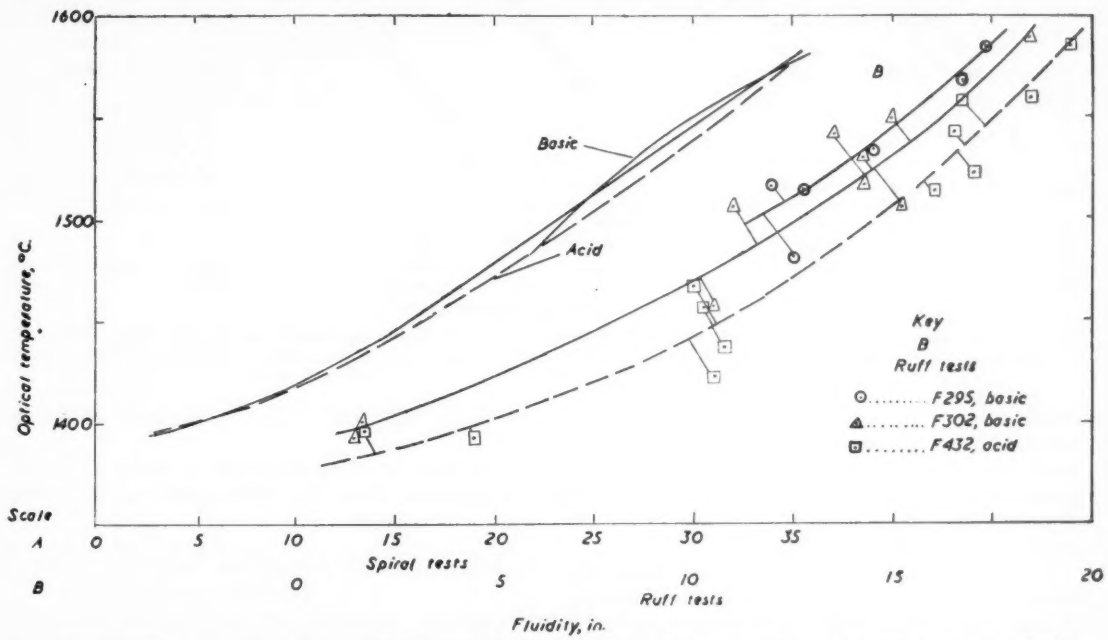


FIG. 8— Optical temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 1 : (A) Spiral tests ; (B) Ruff tests

STEEL NO. 2 (SI–NI)

This composition was found by Taylor and others, to give an extremely fluid steel, and for that reason was included in this series of tests. Four heats were made from this steel and the details are given below.

Heat F. 312 (Basic)

The basic lining of the furnace was very badly attacked and the range of temperature obtained was less than that required.

Heat F. 444 (Acid)

This heat was quite normal.

Heat F. 858 (Basic)

For this heat, in the fluidity/temperature determinations a fewer number of observations than normal were obtained, since in the remaining tests, owing to the fluid nature of the steel, the reserve of molten metal in the heat proved to be insufficient to supply the full length of spiral possible.

Heat F. 884 (Basic)

Again a number of fluidity trials were lost because of the high fluidity of the steel, which produced fashes between the turns of the spiral in spite of special care taken to avoid their occurrence. There was a considerable amount of slag present, which gave a false appearance of skulling.

Relation between Immersion and Optical Temperatures

Each heat is plotted separately (as A, B, C and D) in Fig. 9. Each shank would seem to be consistent in itself, but to differ in its emissivity from other shanks from the same heat. There is no gradual alteration in emissivity with time, the slopes of the curves for different shanks not being changed in a regular manner. The steel would seem to have been altered by its attack on the basic lining of the melting furnace, as the steel itself shows little variation when in the neutrally lined shank. The attack is probably due to the

XUM

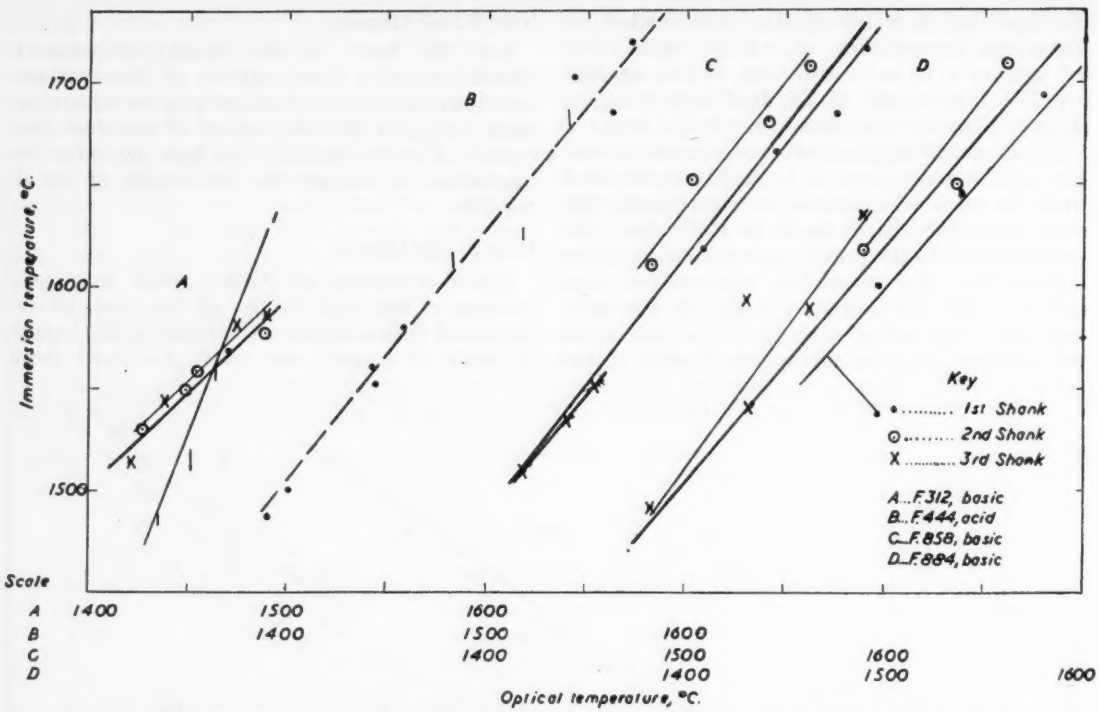


FIG. 9—Immersion temperature *versus* optical temperature. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 2 : (A) Heat F.312—basic ; (B) heat F.444—acid ; (C) heat F.858—basic ; (D) heat F.884—basic

acidic nature of the steel, as the steel made in the acid-lined furnace does not show such marked variation. All the points for the basic heats are given (by A) in Fig. 10 and for the acid heat (by B) in Fig. 10. The average curves in the two cases are given (by C) in Fig. 10.

It is hardly possible to draw a mean line through the points for each heat, and accordingly, all the points have been considered together to find the scatter. The standard deviation of the optical temperature readings for the basic heats is 17·5° C., and that for the acid heat is much less, 10·5° C.

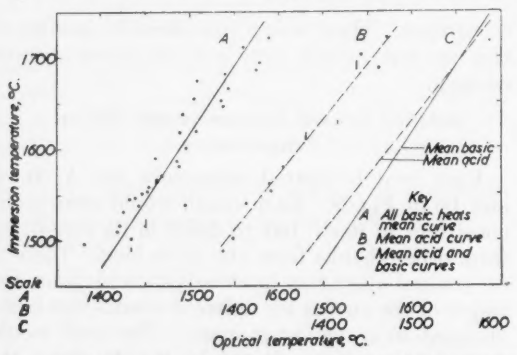


FIG. 10—Immersion temperature *versus* optical temperature. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 2 : (A) Mean curve of basic heats ; (B) mean curve of acid heat ; (C) mean curves for acid and basic heats

The acid steel again shows a lower apparent temperature than the basic steel at the same true temperature.

Relation between Immersion Temperature and Fluidity

Curves A, B and C, Fig. 1, show the points for the Spiral tests for each heat ; only three points were available for heat F.884, and it has not been plotted separately. The scatter is rather large, but each shank appears to be reasonably consistent. The points for the basic and acid heats are plotted (as A and B) in Fig. 12, and the average curves so found are shown (as C) in Fig. 12. As shown in Table II, the standard deviation of results is rather large, but this is probably mainly due to the flat nature of the curves, so that any error in temperature measurement will appear as a large error in fluidity. The Ruff test results on this steel are virtually useless because of the extreme fluidity of the steel, the majority of the test moulds being full or overflowing. The fluidity of the acid steel is slightly less than that of the basic steel.

Relation between Optical Temperature and Fluidity

The four heats are treated as in the previous

section, the curves being plotted (as A, B and C) in Fig. 13 and (as A, B and C) in Fig. 14. For the reasons given above, heat F.884 is omitted. The spread of points is seen to be only slightly greater than in the case of the immersion temperatures. At lower temperatures, the fluidity of the acid steel is higher than that of the basic steel at the same apparent temperature,

STEEL NO. 3 (LOW-CARBON)

Two basic heats and one acid heat were made.

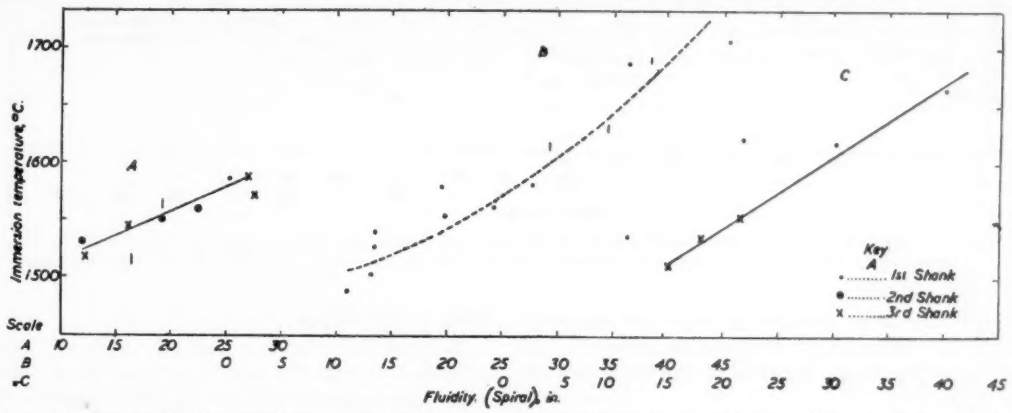


FIG. 11—Immersion temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 2 : (A) Heat F.312—basic ; (B) heat F.444—acid ; (C) heat F.858—basic

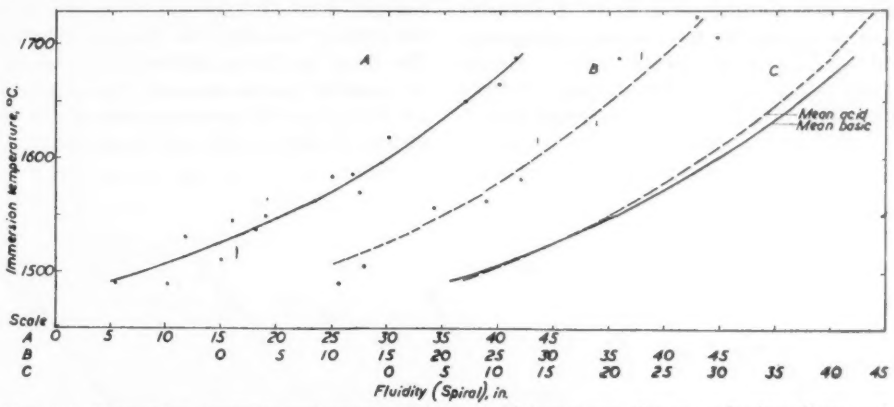


FIG. 12—Immersion temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 2 : (A) Mean curve of basic heats ; (B) mean curve of acid heat ; (E) mean curves for basic and acid heats

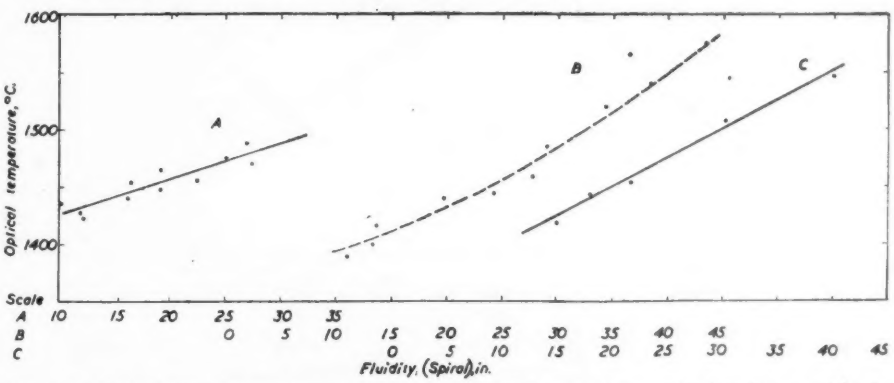


FIG. 13—Optical temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 2 : (A) Heat F.312—basic ; (B) heat F.444—acid ; (C) heat F.858—basic

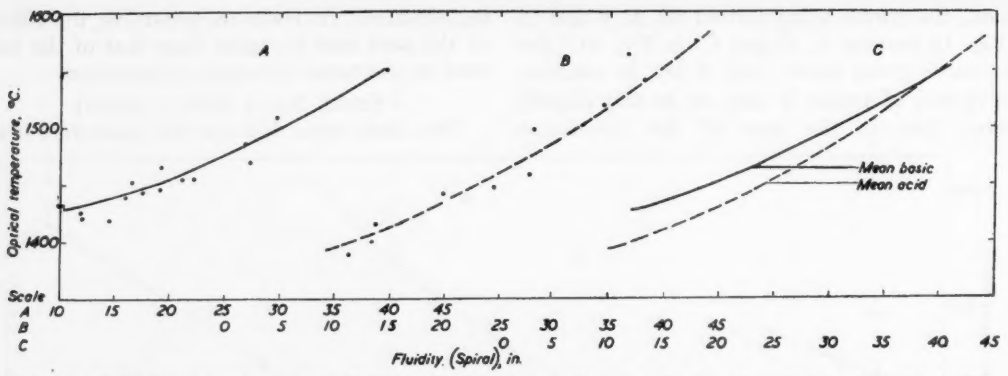


FIG. 14—Optical temperature *versus* fluidity. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 2 : (A) Mean curve for basic heats ; ½(B) mean curve for acid heat ; (C) Mean curves for acid and basic heats

The experimental conditions were not as satisfactory as desired, but the results, however, are of value. The main features of the trials are set out below.

Heat F. 319 (Basic)

The silicon and manganese content of this heat was much lower than for the corresponding steel considered by Taylor and others. Four shanks of steel were used, the number of pairs of tests cast from each being 3, 3, 2 and 5, respectively, making 13 Ruff and Spiral moulds cast in all.

Heat F. 333 (Basic)

Because of the low silicon content it was intended to run the heat cold, and then quickly raise the temperature for the last shank. The quantity of the steel for the last shank, however, was insufficient for effective heating in the furnace, and the delayed holding therein merely burnt off the silicon, causing the steel to rise when cast. The drop in silicon content is shown in Table I. As the first shank was cool, the steel was almost solidifying as the last tests were about to be cast, and the Spiral mould only was cast.

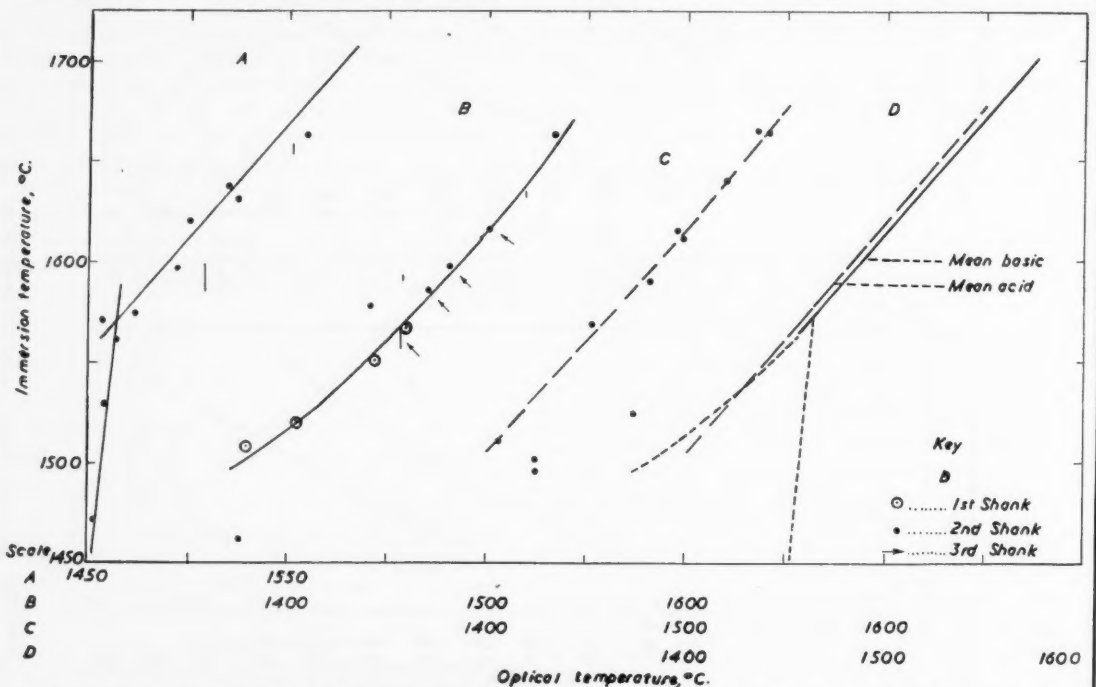


FIG. 15—Immersion temperature *versus* optical temperature. Acid heats are shown as broken lines and basic heats as full lines. Steel No. 3 : (A) Heat F.319—basic ; (B) heat F.333—basic ; (C) heat F.900—acid ; (D) mean curves for acid and basic heats

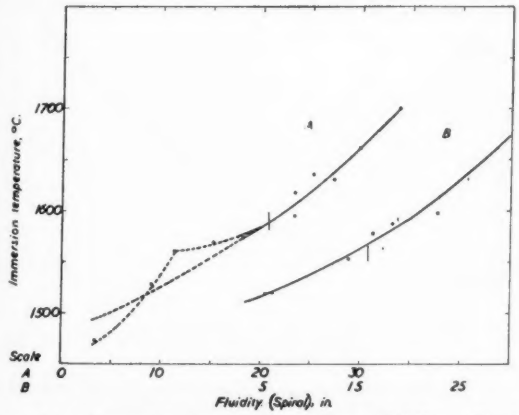


FIG. 16—Immersion temperature *versus* fluidity. Steel No. 3: (A) Heat F.319—basic; (B) heat F.333—basic

Heat F. 900 (Acid)

This was made in an acid lining. Four shanks were poured, giving 4, 3, 3 and 2 sets of tests, respectively. As the fluidity tests were unsatisfactory, the results are confined to comparisons of immersion and optical temperature.

Relation between Immersion and Optical Temperatures

The results for each heat are given (by A, B and C), in Fig. 15, and the curves only for all three heats (by D) in Fig. 15. When examined in detail, there is a slight indication of variation of emissivity from shank to shank, but this is hardly significant.

The curve for heat F.319 falls into two distinct parts. The curve for heat F.333, which was made to establish this double curve, does not show a kink, but the points on curve B, in Fig. 15, shown ringed, are from the first shank and have a silicon content of 0·18% as against 0·06% for curve A. The points indicated on curve B by arrows, were obtained from the last shank, in which the steel was rising. The lowest four points on curve C are below the temperature at which the kink occurs in curve A, and show considerably more variation than the other points on curve C. The standard deviations in the three heats, ignoring one point on curve A, two on curve B and three on curve C which are definitely off the general curve, are 6·3°, 4·9° and 3·1° C., respectively. Including all the points from the basic heats, the standard deviation from the mean curve is 11·3° C.

Relation between Immersion Temperatures and Fluidity

Again there is little variation from shank to shank, and the two heats are shown (as A and B) in Fig. 16. The fluidity curve for heat F.319 which follows the points, again shows a kink, and divides

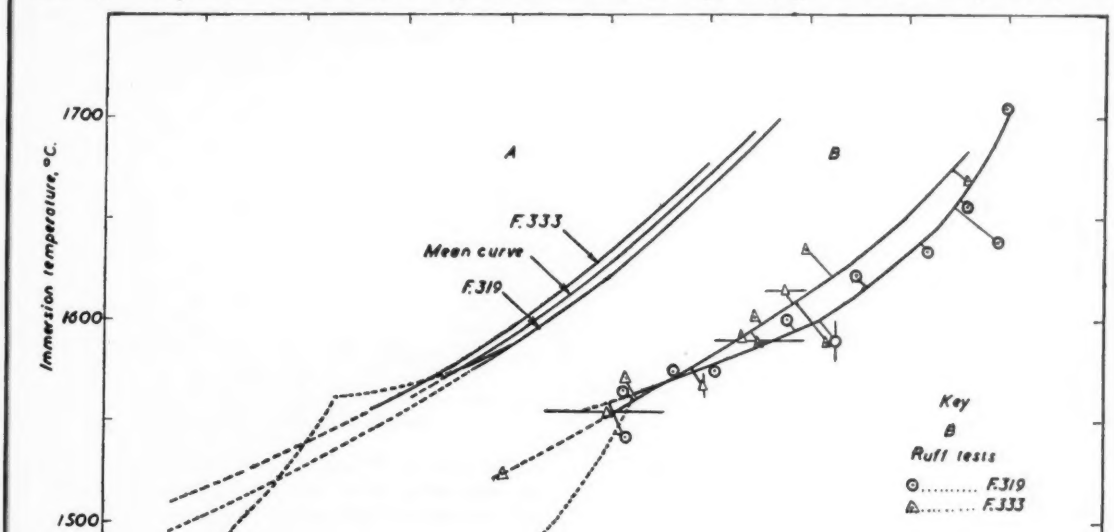


FIG. 17—Immersion temperatures *versus* fluidity. Steel No. 3: (A) Spiral tests; (B) Ruff tests

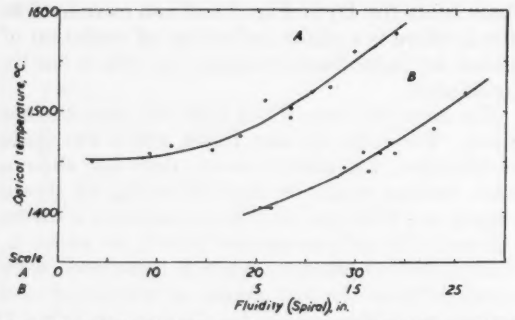


FIG. 18—Optical temperature *versus* fluidity. Steel No. 3 : (A) Heat F.319—basic ; (B) heat F.333—basic

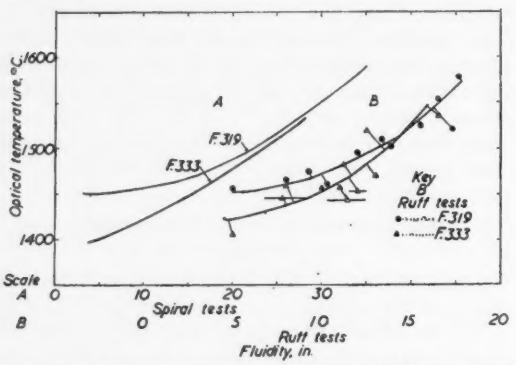


FIG. 19—Optical temperature *versus* fluidity. Steel No. 3 : (A) Spiral tests ; (B) Ruff tests

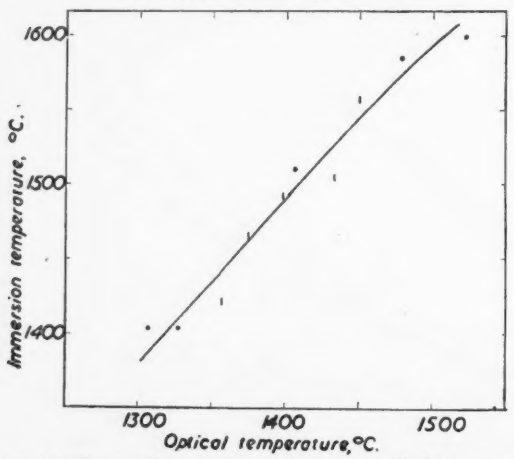


FIG. 20—Immersion temperature *versus* optical temperature. Steel No. 4 : Heat F.324—basic

into two separate parts. The standard deviation for this curve is 0·9 in. Because of the peculiar nature of the lower part of the curve, a second line, which is a smooth continuation of the upper portion is also shown, but the standard deviation for this modified curve will accordingly be greater than 0·9 in. The standard deviation from the curve B in Fig. 16, is 1·25 in., which is much

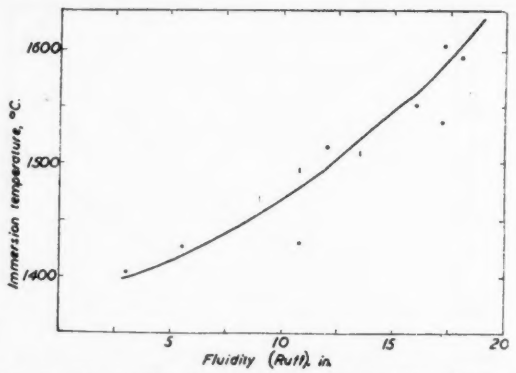


FIG. 21—Immersion temperature *versus* fluidity (Spiral). Steel No. 4 : Heat F.324—basic

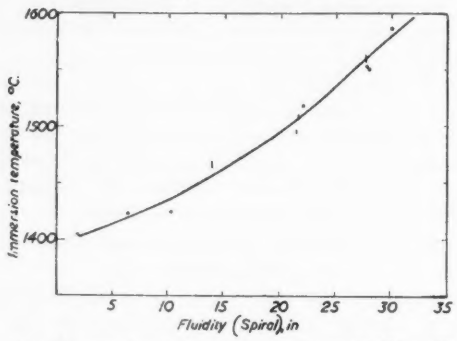


FIG. 22—Immersion temperature *versus* fluidity (Ruff). Steel No. 4 : Heat F.324—basic

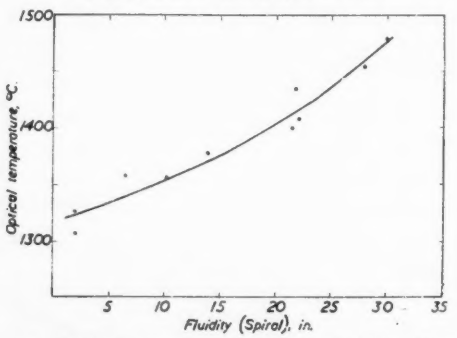


FIG. 23—Optical temperature *versus* fluidity (Spiral). Steel No. 4 : Heat F.324—basic

larger than in the other cases, and may be taken as confirming that the condition of the steel is not consistent throughout the heat.

The two curves from Fig. 16 are given (by A) in Fig. 17 for comparison with curve B, in Fig. 17, showing the Ruff test results to a length scale which has been doubled. A mean curve between the two shown (by A) in Fig. 17, not including points below the kink, gives a standard deviation of over 1·6 in., which again indicates that the points are not consistent.

The Ruff curves, especially for heat F.333,

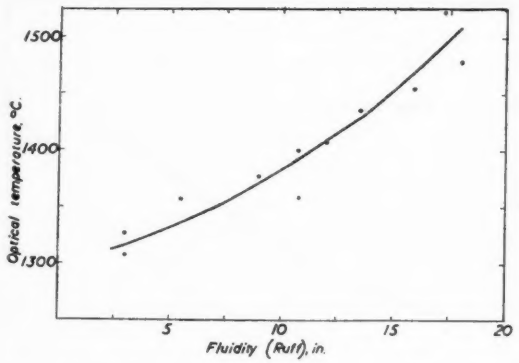


FIG. 24—Optical temperature *versus* fluidity (Ruff). Steel No. 4: Heat F.324—basic

again show considerable scatter, the region of anomalous results being indicated by a dotted line through the experimental observations.

Relation between Optical Temperature and Fluidity

Curves A and B of Fig. 18 show the optical-temperature against Spiral-test results for the two heats, and curves A and B, Fig. 19, give the comparison of Spiral and Ruff mould results, the Ruff length being drawn to double the scale. There is no kink in any of these curves. Apparently, in this case, the change in the condition of the steel of heat F.319 which affected the fluidity, also influenced the emissivity in such a manner that the discontinuous alterations in fluidity were automatically compensated for by alterations in the optical-temperature readings.

If the kinks in the immersion/optical temperature curves and immersion-temperature/fluidity

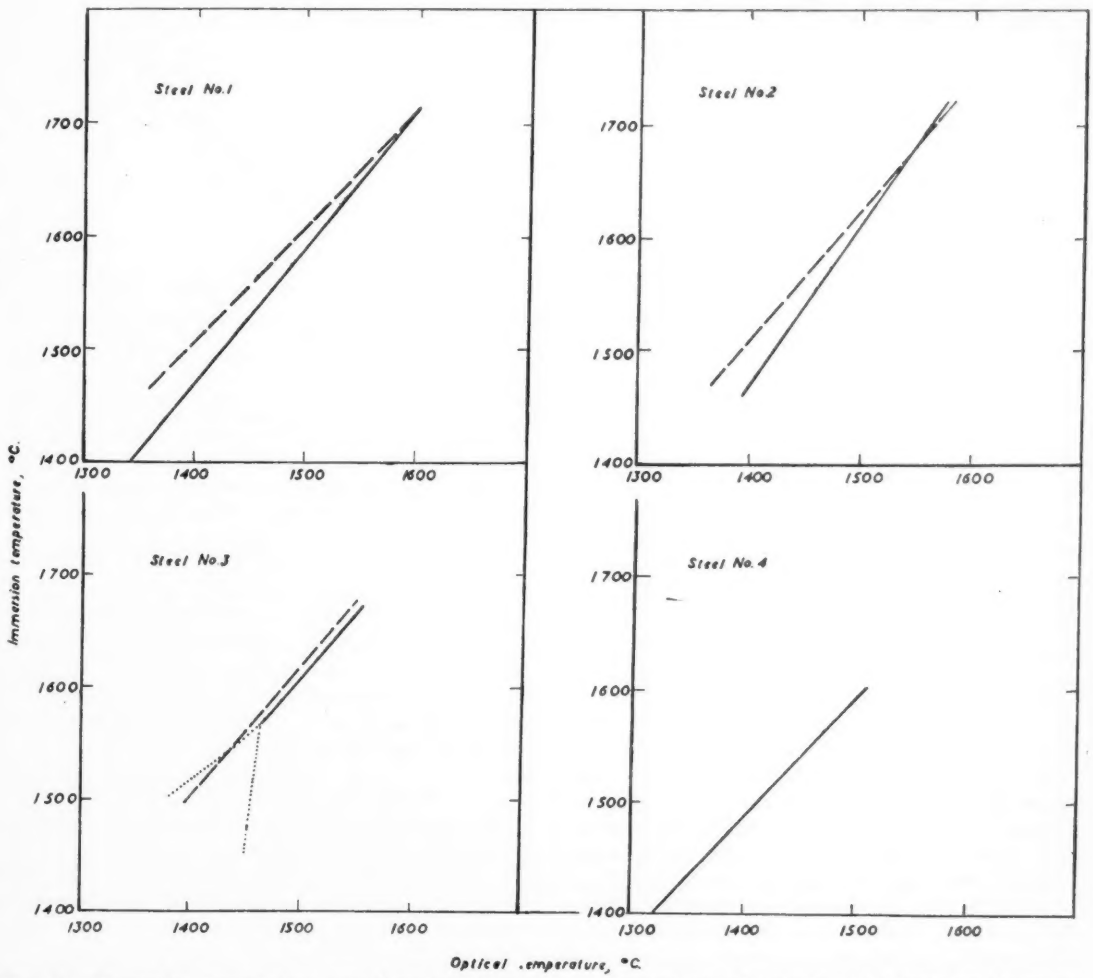


FIG. 25—Immersion temperature *versus* optical temperature. Comparison of 4 steels. Acid heats are shown as broken lines and basic heats as full lines

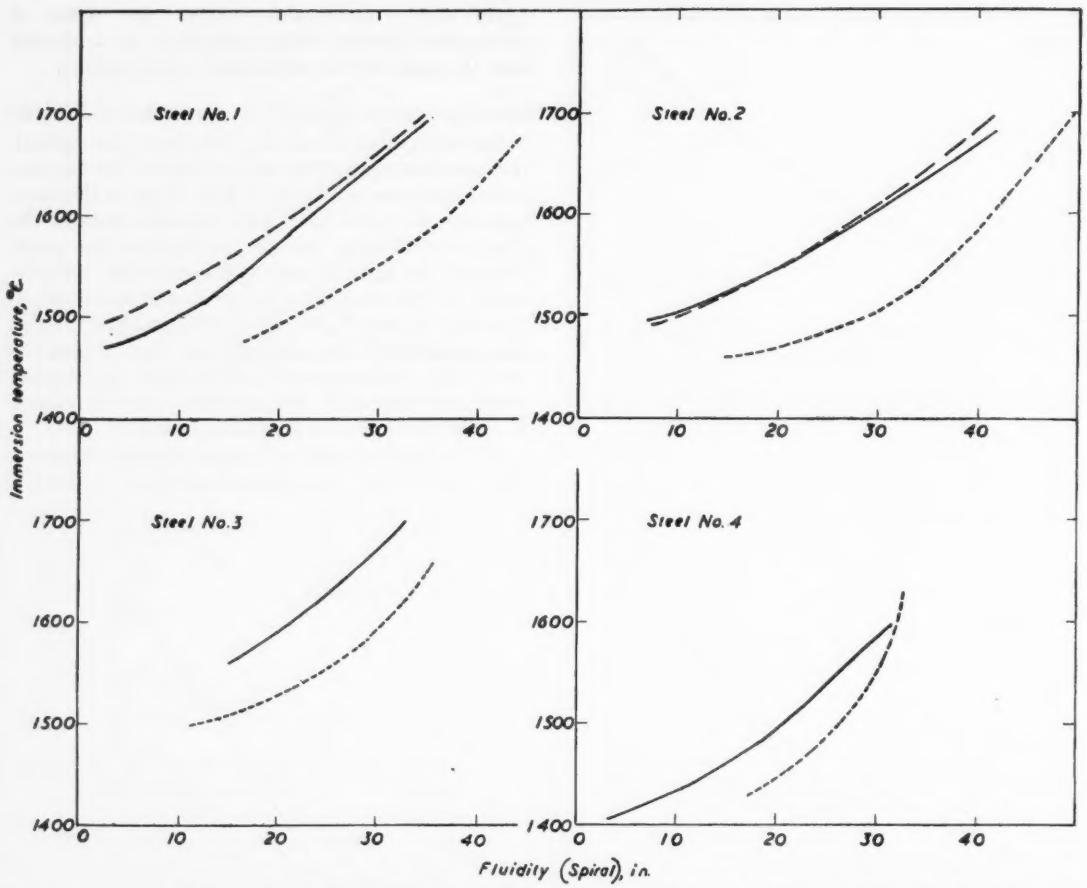


FIG. 26—Immersion temperature *versus* fluidity. Comparison of 4 steels. Acid heats are shown as broken lines and basic heats as full lines. Curves from Taylor and others are shown dotted

curves in Figs. 15, 16 and 17 are due to errors in observation, two possibilities occur ; either the immersion-couple readings are at fault or the optical and fluidity observations are both in error by amounts which cancel each other. At the time of the experiment it was noticed that there was a change in the appearance of the metal when the temperature at the kink was reached during the casting of the last shank. It would therefore be anticipated that the subsequent optical readings would not follow the previous relationship. However, this is not so, as the optical-temperature/fluidity relationship follows a smooth curve throughout. Alternatively, the immersion-couple temperatures may be wrong, but no obvious error was apparent, and in the curve of immersion temperature against optical temperature the deviations from the normal—as much as 100° C.—are so great that they are very unlikely to arise from experimental errors. At the low temperatures at which the kink occurs, it is not expected that any serious change in the calibration of the couple, due to contamination, could occur. Furthermore, all the readings taken immediately before and after the change were taken with the same couple.

The standard deviation in curve A, Fig. 18, is 1·8 in. ; this is high because of the flat nature of the lower end of the curve and, excepting this part, the standard deviation is 1·4 in. The value is considerably greater than that given in curve A, Fig. 16. Curve B, in Fig. 18 gives a standard deviation of 1·28 in., which compares with 1·25 in. from curve B, in Fig. 16.

STEEL NO. 4 (13% MANGANESE)

A normal manganese steel was included in the series for comparison. Only one heat was made as this gave a satisfactory series of tests, and it was not possible, because of the nature of this steel, to compare the effects of basic and of acid linings. The details of the composition are given in Table I. A peculiar feature of the Spiral tests was that the ends of the spirals did not fill the

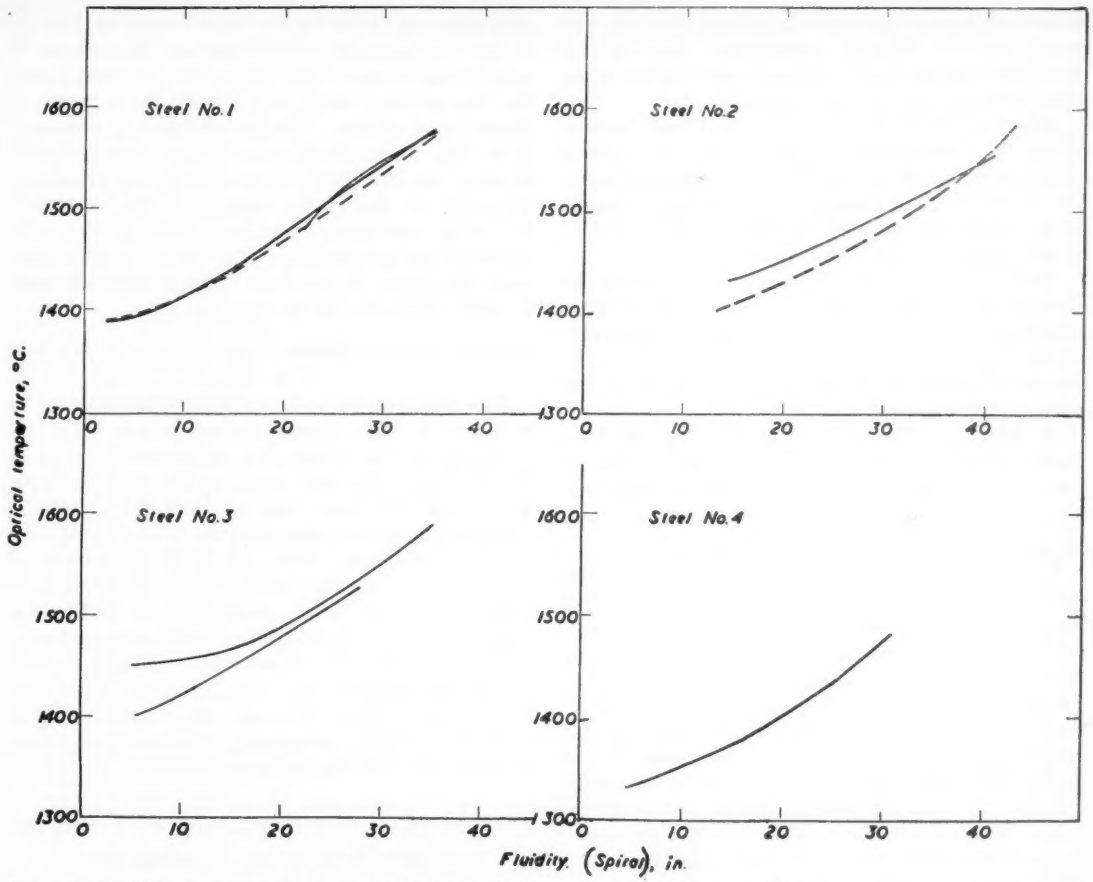


FIG. 27—Optical temperature versus fluidity. Comparison of 4 steels. Acid heats are shown as broken lines and basic heats as full lines

cross-section of the moulds, but the last inch or so was rounded and of reduced section, apparently due to a surface film being pushed along by the steel. The steel was cast in three shanks, supplying 4, 5 and 3 sets of tests, respectively.

Relation between the Immersion and Optical Temperatures

There would seem to be no effect of different shanks on the emissivity. The points are plotted in Fig. 20.

Relation between Temperature and Fluidity

The relationships between the immersion temperature and fluidity are shown in Figs. 21 and 22 for the Spiral and Ruff tests, respectively. The length scale for the Ruff tests (Fig. 22) is again double that for the Spiral tests, and the resulting curves are of similar shape, indicating that the two types of tests are in reasonable agreement, although the scatter for the Ruff test is considerably greater than in the case of the Spiral tests.

The relation between optical temperature and fluidity is shown in Figs. 23 and 24, where similar remarks apply.

COMPARISON OF THE BEHAVIOUR OF DIFFERENT STEELS

In the previous discussion the validity of the mean curves drawn for each heat, and for groups of heats of the same steel made in a similar type of lining, has been considered. These mean curves are now grouped together according to the properties studied, in Figs. 25, 26 and 27. Figure 26 also contains curves of four similar steels extracted from the paper by Taylor and others.[1]

Relation between Immersion and Optical Temperatures (Fig. 25)

Where results on both acid and basic steels are available, the acid has a lower apparent temperature than the basic steel at the same true temperature; the difference in two cases decreases with increasing temperature, and vanishes at approximately 1700° C. The difference in emissivity causing this change has been discussed in a

separate paper,[4] and the difference between the mean curves relating immersion and optical temperatures for acid and basic steels of the same composition, has been shown to be real.

All the curves lie in a comparatively narrow band, the maximum variation in the optical readings being 40° at 1470° C., and the minimum, 25° at 1530° C. Although this variation is small, it is significant in view of the small spread of points found in individual heats.

Because of this small variation it would be unwise to attempt, on the slender data here available, to determine the effect on emissivity of the various elements present in the steel ; the matter has been dealt with in more detail in the paper mentioned above.[4]

In Fig. 26, the authors' results on the four steels studied are compared with the results on four steels of approximately the same composition investigated by Taylor and others.

Authors' Curves

Where acid and basic heats are available, the acid is seen to be less fluid than the basic steel over the greater part of the temperature range.

The least fluid steels are the basic steel No. 3 (low carbon), and the acid steel No. 1 (2% Cu). These are followed closely by basic steel No. 1. Steel No. 2 (Si–Ni) is much more fluid, except below approximately 1500° C., and steel No. 4 (13% Mn) flows more readily still, presumably because of its much lower solidification temperature. The solidification (zero fluidity) temperatures of the other steels are very similar to each other.

Curves from Taylor, Rominski and Briggs

In Fig. 26 the curves given by these American authors fall to the right of those obtained in the present work, that is, they show much higher fluidity at the same temperature. An approximate relation, not true throughout, however, is a difference of 10 in. in the Spiral tests or 100° C. in the temperature. The standard deviations of spiral lengths are of the order of 2·5 in. maximum for the authors' tests and 1·8 in. for a curve of Taylor and others. The latter value is obtained from Fig. 23 in their paper, which was included to show the standard of reproducibility obtained. The shift in the curves would not be explained by such variation therefore, and attention is drawn to the possibilities of errors in temperature measurements. A check on the two methods used is dealt with later in Section II.

Relation between Optical Temperature and Fluidity (Fig. 27)

The relation between acid and basic heats is reversed in this Figure as compared with the previous one for immersion temperatures. Steel No. 1 shows the acid steel to be slightly more fluid than the basic, and in steel No. 2 this is definitely so up to a temperature of approximately 1545° C. (optical). Although both the curves of steel No. 3 are for basic heats, they diverge considerably at lower temperatures, indicating some peculiarity of the steel at these temperatures.

The general order of increasing fluidity—steels No. 3 (low carbon), No. 1 (2% Cu), No. 2 (Si–Ni), No. 4 (13% Mn)—remains unchanged, though steel No. 4 shows a tendency to cross the curves for steel No. 2 at higher temperatures.

Section II—Considerations of Temperature-Measuring Technique

As stated in Section I, the discrepancy between results obtained by the authors and those reported by Taylor and others, led to doubts of the accuracy of the measurement of temperature. A series of experiments was carried out, therefore, to compare the temperature measurements obtained by the authors' equipment with an assembly approxi-

TABLE IV—*Details of Thermocouple Technique*

	Couple as Described by Taylor, Rominski and Briggs	"T.R.B." Couple	Authors' Couple
Thermocouple ...	18 gauge W–Mo wire ...	26 gauge Pt/Pt–13%-Rh wire	26 gauge Pt/Pt–13%-Rh wire
Inner sheath	Small fused-quartz tube 30 in. long	Fused-quartz tube, 14½ in. long, thickness of wall $\frac{1}{16}$ in., outside dia. $\frac{3}{8}$ in.	None
Outer sheath	Quartz tube, 10 in. long, 1⅛ in. outside dia., wall thickness 0·15 in. min.	Quartz tube 12½ in. long, 1⅛ in. outside dia., wall thickness 0·125 in.	Quartz tube 12 cm. (4¾ in.) long, 7 mm. (¼ in.) outside dia., wall thickness ¾ mm. ($\frac{1}{32}$ in.)
Preheat	8 in. of the assembly preheated in a small muffle furnace at 900° C.	The couple was held in a clamp round the outer tube, and was preheated to 900° C. in a small electric tube furnace	None
Depth of immersion of couple	2½ in.	3⅜ in.	3½ in.

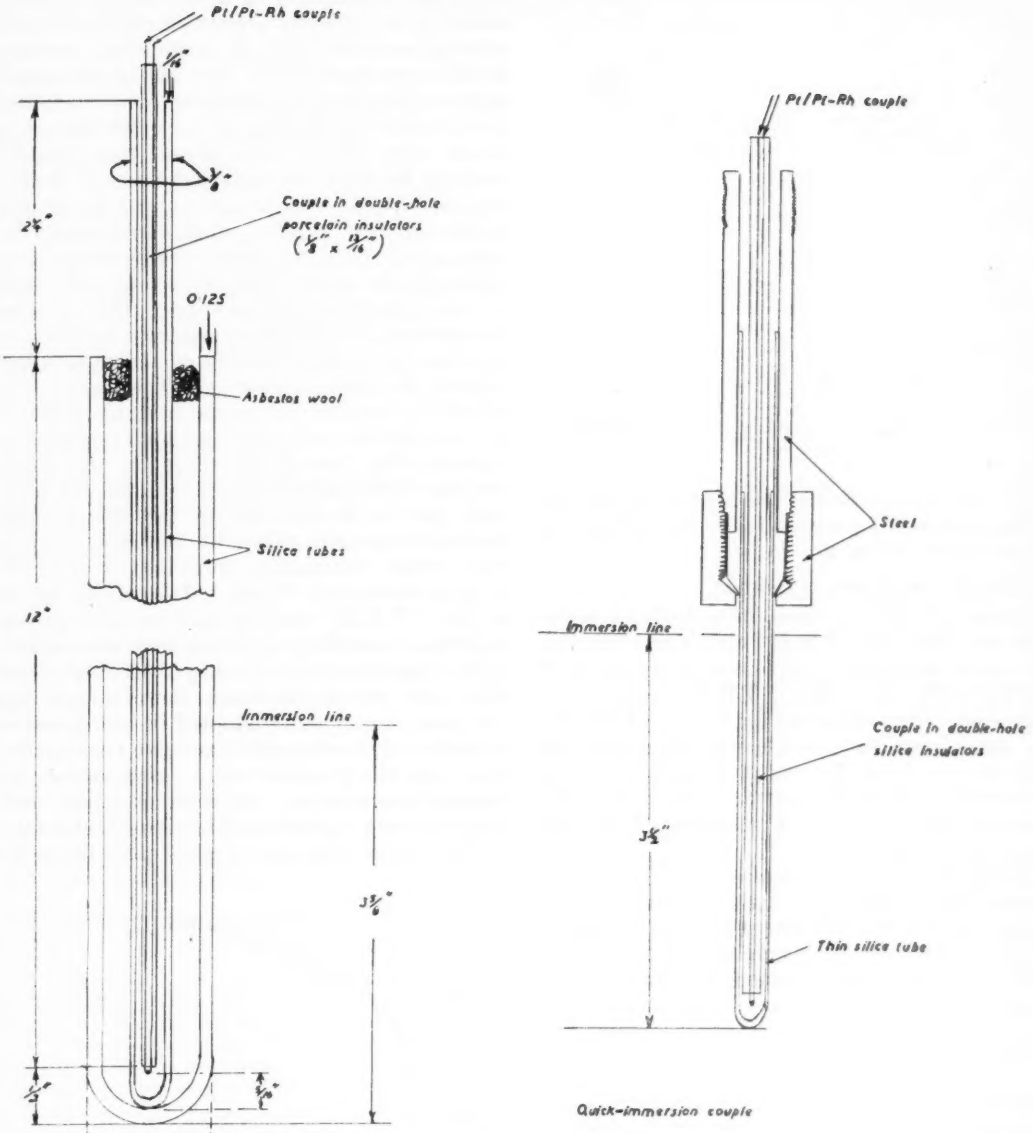


FIG. 28—Details of quick-immersion and "T.R.B." thermocouples

mating to that reported by Taylor and his co-workers.

Experimental Comparison of the Technique of Temperature Measurement as Practised by the Authors and by Taylor, Rominski and Briggs

A thermocouple assembly was made to imitate as far as practicable that used by Taylor and others, and it is referred to in this description as the "T.R.B." couple. It is illustrated in Fig. 28. The details of technique are compared in Table IV, which also includes details of the thermocouple used by the authors.

In the original American experiments the couple was immersed continuously, the furnace maintained at a steady temperature, and the power input kept high at pouring to clear the slag and mix the bath. This technique was used when the steel was cast at a rising series of temperatures. During a falling series the bath was allowed to cool with little or no power input, but the power

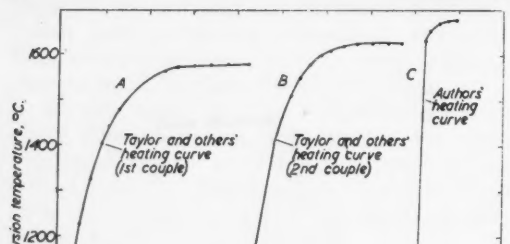


FIG. 29—Comparison of speed of response of thermocouples

input was again raised at the instant of pouring.

The authors' couple was of the quick-immersion type,[5] shown in Fig. 28.

Method of Comparison of Couples

Three preliminary trials in a bath of liquid steel, two with the " T.R.B." couple and one with the quick-immersion couple are shown (as A, B and C) in Fig. 29. The " T.R.B." couple was immersed, after preheating for 10 min. at 900° C., the quick-immersion couple was immersed cold and the e.m.f. of the couple in each case was measured by a potentiometer. There is a pronounced difference in the response of the two types of couple.

On the basis of these results a further test was carried out in the 2-cwt. high-frequency furnace using both types of couple, the " T.R.B." couple preheated and the quick-immersion one starting cold ; the latter being immersed approximately 1 min. later than the former. Both sets of readings were taken on the same potentiometer. The results are shown in Fig. 30.

Comparison of Results by the Two Methods

Towards the end of the immersion period of 3½ min., the quick-immersion couple showed signs of failing. This was not surprising as the immersion time was unduly long for this type of couple. It was not possible to control the furnace to be perfectly steady, and a slow rise in temperature was shown by both couples.

If the wall of the " T.R.B." couple had been as thick as that used by Taylor and others, it is reasonable to suppose that the response would have been even slower.

Accuracy of Temperature Measurement by the Two Methods

The long immersion time of the couple used by the American investigators should give a good value of the temperature if the furnace is maintained at a perfectly steady temperature until equilibrium is reached. It is doubtful, however, if this is ever attained. The lag of the couple indicated in the experimental comparison, showing 15° C. below the reading of the quick-immersion couple even after 3 min., is so great that the readings are liable to considerable error. As it is claimed that 20 moulds are poured in 30 min. (Reference 1, p. 17), the couple had a maximum time of 1½ min. to attain temperature, even assuming the rise of temperature at each stage to take place instantaneously. The recorded temperature was therefore likely to be low. In addition the power input was increased to a high rate at the time of pouring. This would have raised the temperature of the bath by, possibly, a considerable amount, without showing a corresponding immediate effect on the couple reading. With maximum power input, the measured rise in temperature of the 2-cwt. high-frequency furnace, taken by the quick-immersion couple continuously immersed in the bath, is approximately 1° C./sec. The values for lag of the " T.R.B." couple used in the present experiment are likely to be less than those existing in the original set-up as used by Taylor and others. Also, the use of the thick thermocouple wire (18 gauge, *i.e.*, 1 mm.) adopted in the American experiments, would cause excessive heat conduction from the junction and a corresponding low temperature reading. An error of a high order does not seem unreasonable in view of these facts.

The rapid response of the quick-immersion

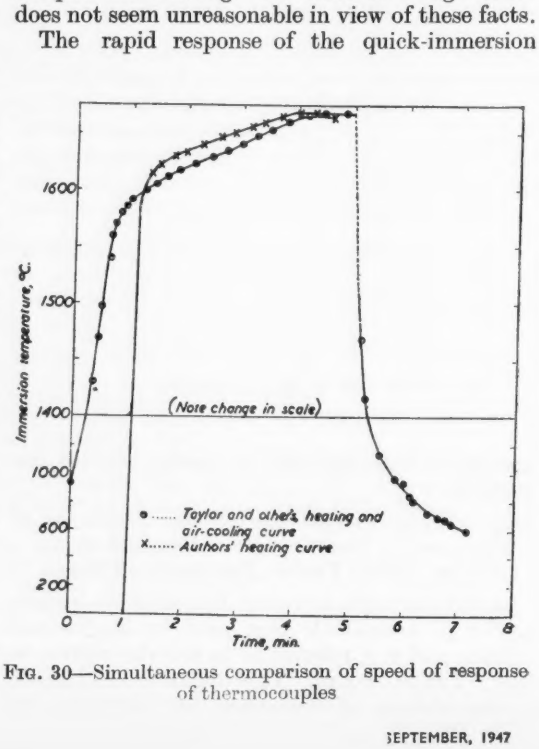


FIG. 30—Simultaneous comparison of speed of response of thermocouples

couple would indicate that the temperature indicated by an apparently steady reading after about 20 sec. is near the correct one, the slow rise with time, of temperature indicated by the couple, being most likely due to an actual alteration in temperature of the furnace.

Both types of couple are liable to chill the immediately surrounding steel and so give rise to a low reading. This effect will be greater with the American couple than with the quick-immersion because of its greater mass. It is doubtful, however, in the case of the quick-immersion couple, whether this effect is material except at temperatures near the freezing point, where equilibrium in the steel may be approached more slowly.

Survey of the Two Methods

The "T.R.B." couple tested shows a pronounced lag as compared with the quick-immersion couple, and this lag would probably be increased with the set-up as arranged by Taylor and others. In the case determined, the lag was of the order of 15° C. after 1½ min.

The method adopted, by Taylor and others, of increasing the current in the furnace before pouring, is liable to further error in conjunction with the lag in the thermocouple. With a time of increased heating of 10 sec. an added difference of temperature of 10° C. is reasonable. The estimated error between the readings of Taylor and others and those which would have been obtained with the quick-immersion couple is of the order of not less than 25° C.

Conclusions

A comparison of the Ruff fluidity test and the type of Spiral fluidity test used by Taylor, Rominski and Briggs has been carried out, and although the type of fluidity/temperature relation in the two cases is of approximately the same form, the Spiral test is subject to much less error than the Ruff test.

A difficulty with the Spiral test is the precision needed in making and assembling the mould. Any distortion, or unequal clamping pressures for the two halves of the mould, leads to leakage of the metal between the turns of the spiral during casting and so to the failure of the test.

The curves obtained by plotting the length of the fluidity test-piece against temperature, as measured by immersion couple or optical pyrometer, are generally of a similar form, except in one case (heat F.319) which has been specially considered. The accuracy of the readings obtained by the optical pyrometer, as shown by the scatter of points round the mean fluidity/temperature curves, is rather less than that of the readings given by the immersion couple. This is not surprising, as the optical readings are difficult to take in the extremely short time available, but the error is much less than might have been expected and is widely different from that given by other investigators, as for example by Taylor and others (Reference 1, p. 21). No doubt the experience and the ability of the observer have a considerable effect in reducing this error. In the case of heat F.319, there was a kink in the curve of fluidity/immersion-couple-temperature which was not present in the fluidity/apparent-temperature curve. No reason for the existence of this kink has so far been found.

The method of casting the tests from a hand-shank, whilst not ideal from an experimental standpoint, is in line with a considerable amount of foundry practice, and the results should be directly applicable to these cases.

There is a wide discrepancy between the results for fluidity given in this paper, and those obtained by Taylor, Rominski and Briggs. The discrepancy seems too large to be due to a real difference in the nature of essentially the same steels, or even to the sum of small experimental or equipment errors. A possible explanation is the difference in the methods of measuring temperature, errors of at least 25° C. being conceivable using the methods of Taylor and his co-workers.

The order of merit for fluidity of the four steels considered is, however, similar to that obtained in the American work.

The validity of the differences in the curves for different types of steel, or for different conditions of the same steel, has been considered. A full discussion of certain of the curves has been given in the complementary paper on the emissivity of molten steel,[4] where it is shown statistically that the differences, even when small, are usually significant.

A striking feature of the results has been the comparison between steels of nominally the same composition when made in an acid and in a basic lining. It is an accepted fact in foundry circles that acid steel is more fluid than basic steel. The present work would seem to show that while this may be true when the temperatures are obtained by visual means, the converse holds if the true temperature is considered.

There have been a number of examples in the present work, notably so in the case of heat F.319, in which a change in fluidity of the steel has been accompanied by changes in emissivity. A satisfactory correlation between emissivity of the steel and its fluidity is not possible from the available data, and it is not clear whether the particular examples given are indicative of a more general law or are merely fortuitous.

Acknowledgments

The authors are indebted to the Directors of Messrs. Hadfields, Ltd., for permission to publish this paper, and would particularly wish to tender their thanks to Mr. W. J. Dawson, the Chairman of the Steel Castings Research Committee, for his encouragement of the work. They also thank Messrs. W. J. Todd and J. D. Cresswell for their valuable help.

References

1. H. F. Taylor, E. A. Rominski and C. W. Briggs: *Transactions of the American Foundrymen's Association*, 1941, vol. 11.
2. R. J. Sarjant and T. H. Middleham: *Iron and Steel Institute Special Report No. 23*, 1938, pp. 45–60.
3. E. C. Kron and C. H. Lorig: *Transactions of the American Foundrymen's Association*, 1940, vol. 47, p. 606.
4. D. Knowles and R. J. Sarjant: *Journal of the Iron and Steel Institute*, 1947, vol. 155, April, pp. 577–592.
5. F. H. Schofield and A. Grace: *Iron and Steel Institute Special Report, No. 25*, 1939, pp. 239–264.

XUM

ANGLO-SAXON CUTLERY*

By Dr. H. R. Schubert†

SYNOPSIS

Despite the decline of civilization which followed the Anglo-Saxon invasions after the departure of the Romans, ironworking in England did not cease, as is witnessed by the abundance of iron weapons and implements found in graves of pagan Saxondom of the fifth to seventh centuries. In the Christian era the monastic houses were the centres of learning, arts, and crafts, and an English historian of the eighth century, the Venerable Bede, tells of monks skilled in the forging of iron. About A.D. 670 England emerged from a primitive civilization to a culture of the highest standard, by far surpassing the other countries of contemporary Western Europe. One of the centres of this culture was the kingdom of Northumbria, which extended from the Humber to the Firth of Forth and included the major part of Yorkshire. In this kingdom, in addition to learning and the arts, the manufacture of cutlery attained a very high standard, and knives were sent abroad as special gifts, as is mentioned in letters written between A.D. 732 and 764. In these letters the knives are described as being manufactured in a special fashion customary to the country. Compared with two large knives of the "Hurbuck" type (Hurbuck was a farm in County Durham) and a third, found at Sittingbourne in Kent but evidently of Northumbrian manufacture, the knives mentioned in the letters had straight backs, but sloped slightly towards the point, and they were ornamented by inlay with copper, brass, or white metal. The designs used for ornamentation were probably vine-scrolls, motifs which are characteristic of Northumbrian art.

IN Roman Britain, iron was mined and worked in large quantities and in many parts of the country, such as the Forest of Weald in Sussex and Kent, the Mendips, the Forest of Dean, Monmouthshire and Glamorgan, the Midlands (near Stratford-on-Avon), and the north (in Yorkshire, near Bierley and Bradford[1]). It is much more difficult to trace ironworking in the post-Roman era; the last Roman legions left shortly after A.D. 400, and, in the course of the fifth century, Anglo-Saxon invasions and settlements took place. The fifth and sixth centuries, when civilization was at its lowest standard, are usually called the Dark Ages of early English history.

[1] R. G. Collingwood and J. N. L. Myres, "Roman Britain and the English Settlements," p. 69. Oxford, 1936; R. E. M. Wheeler, "Prehistoric and Roman Wales," p. 272. Oxford, 1925; T. D. Kendrick and C. F. C. Hawkes, "Archaeology in England and Wales," pp. 273–274. London, 1932; F. and H. Wragg Elgee, "The Archaeology of Yorkshire," p. 158. London, 1933.

Whatever the decline of civilization in general, it did not extend to ironworking. The smith was held in high esteem by all Teutonic peoples, and his produce was needed for armament, agriculture, and domestic use just as much as it was before the departure of the Romans. Consequently, iron weapons and implements are frequently found in Anglo-Saxon graves of the pagan period (from the fifth to the seventh centuries) all over the country. There are implements for *agricultural* purposes such as scythes and sickles, and shears resembling in form the sheep-shearing implements of the present day. There are also articles for *domestic* use in house and kitchen, such as bowls, cauldrons, wooden buckets with iron framework, keys, chains with hooks for suspending pots over a fire, and strike-a-lights. *Personal* items found include buckles, brooches with iron pins, and purses with iron mounts and framework. In addition, *craftsmen's tools*, such as chisels, blacksmiths' tongs, and weaving irons for compressing the thread on the loom have also been found. Last, but not least, *weapons*, such as spearheads, shield-bosses to protect the hand holding the wooden shield, swords, and an

* Received 21st December, 1946.

† University of Reading.

abundance of *knives*, small and large, are also amongst the remnants of this period.[2]

Small single-edged knives formed an essential part of personal equipment, and these have been found in the graves of men, women, and children alike. The appreciation of the knife is expressed by the laws enacting that a stolen knife should be returned to its owner, and fixing a fine of the fair amount of 15 solidi for the offence.[3] The knives found in Anglo-Saxon graves vary in length from 3 in. upwards, but are mostly about 6 in. long. Knives of a larger size are called "scramasaxes," or sword-knives, but it is difficult to distinguish them from the knife proper, as one merges into the other as regards size ; both were used as weapons and at meals.[4] The sword-knife or scramasax proper, which may be described as a sort of clumsy carving knife, was a universal implement in Anglo-Saxon England at the beginning of the seventh century.[5]

A primitive North of England smithy is depicted on the famous "Franks' casket," a Northumbrian carving in whale's bone, of about the period A.D. 700, preserved in the British Museum. A portion of this carving, in which Wayland the smith is seen holding his tongs over an anvil, is shown in Fig. 1.[6]

The smiths of pagan Saxondom seem to have been of a rather strong-willed and independent nature, as is shown in the legend of St. Ecgwin. When Ecgwin, Bishop of Worcester, came to Alcester (Warwickshire) around A.D. 700, he found the town peopled with smiths, who refused to listen to his preaching and attempted to drown his voice by the sound of their hammers and anvils.[7]

Compared with the pagan period, archaeological evidence is very much less frequent in the Christian period of Anglo-Saxon history. When Christianity was firmly established, between 650 and 700, the pagan habit of furnishing the graves with objects for the use of the dead ceased, but there is no reason to suppose that the fabrication of arms and ornaments by secular craftsmen came to an end at the request of the Church.[8] The most important centres of the Christian period were the monastic houses. Outside the monasteries craftsmen were trained in almost all branches of work, sometimes working for or with the monks. In the monasteries almost all forms of arts and crafts were practised, as manual labour was a requirement laid down in monastic rules in order to provide a safeguard against idleness.

Many of the Benedictine monks who rose to high-ranking positions divided their time between

FIG. 1—Part of the Franks' casket, depicting Wayland's smithy

prayer and manual labour. Perhaps the best known of these is Dunstan, who lived from 924 to 988 and became Archbishop of Canterbury in his later years. His activity as a smith is crystallized in the legend of his encounter with the Devil at Glastonbury (Somerset), the scene being transferred afterwards to Mayfield (Sussex), where Dunstan's tongs, with which he is supposed to have seized the Devil's nose, and his anvil are still to be seen.[9]

Our knowledge of ecclesiastics skilled in smith's craft is not confined to legends. Real historical evidence is furnished by the Venerable Bede, "the father of English history." In his history of the abbots of the monasteries of Wearmouth and Jarrow-on-Tyne, of which monasteries he was a member until his death in 735, Bede speaks of one of the abbots, Eosterwini, as a man skilled in the forging and shaping of iron with the

[2] G. Baldwin Brown, "The Arts in Early England : Saxon Art and Industry in the Pagan Period" (especially pp. 197–8, 391–2, 395–7, 409–16, 422, 461). London, 1915 ; "British Museum Guide to Anglo-Saxon Antiquities," pp. 57, 63, 68, 70, 84–94. London, 1923.

[3] J. de Baye, "The Industrial Arts of the Anglo-Saxons," p. 27. London, 1893.

[4] G. Baldwin Brown, "The Arts in Early England," pp. 226–7, and Plate XXVIII (selection of knives found in Kent, Leicestershire, Staffordshire, and Yorkshire).

[5] R. E. M. Wheeler, "London and the Saxons," p. 177. London, 1935.

[6] Reproduced from G. Baldwin Brown's "The Arts in Early England," vol. 6, Part I : Northumbrian Art, Plate VII (facing p. 29). London, 1930.

[7] Victoria County History of Warwick, vol. 3, p. 12. London, 1945.

[8] G. Baldwin Brown, "The Arts in Early England : The Life of Saxon England in its Relation to the Arts," new and revised edition, p. 240. London, 1926.

[9] E. Straker, "Wealden Iron" (second edition), p. 31. London, 1931.

XUM

hammer.[10] During the abbacy of Huetbert, early in the eighth century, the monks were noted for bell-founding and metalwork.[11]

Another monastic smith is mentioned by Ethelwulf in a Latin poem, written early in the ninth century, on the abbots of his cell, which was near York and belonged to the monastery of Lindisfarne. The monk mentioned was Cuicuin, and he is praised by the poet for his skill in the shaping of iron with the forge-hammer.[12]

Lindisfarne and its cells were monasteries of Celtic-Irish observance, whilst Jarrow and Wearmouth adhered to the Benedictine rule. We have thus two striking instances of monks excelling in the smith's craft in both the monastic orders to which the monasteries of Britain belonged in those early ages. Also, both places of monastic iron-working mentioned were in the north of England, in the Anglican kingdom of Northumbria, which extended from the Humber to the Firth of Forth between the North Sea and the Pennines.

At this time Anglo-Saxon England attained its ighest standard in learning and the arts, especially in the kingdoms of Kent and Northumbria. It was the age when England, with a suddenness unparalleled in European history, arose from a primitive civilization to become the home of a Christian culture which influenced the whole development of letters and learning in Western Europe.[13] At the end of the seventh century and the beginning of the eighth, England achieved a unique position in learning and the arts, a position supreme in western civilization. This glorious period is marked by the introduction of a quasi-oriental late-antique art with underlying Roman classicism. This developed into a specifically English art, so that the period well deserves the name of an " Anglo-Saxon renaissance."[14]

One of the main instigators of this development was Benedict Biscop, the founder of the monasteries of Wearmouth and Jarrow-on-Tyne. From his various journeys to Rome, Benedict Biscop brought a large collection of books, ecclesiastic vestments, tapestries, and paintings to adorn his new churches. For his buildings he induced masons and glassmakers to come from France. He thus re-introduced the art of glassmaking, known to Roman Britain but then long-since lost. Owing to his unceasing zeal Jarrow and Wearmouth became a focus of learning and art in Northumbria until late in the ninth century, when Danish invaders—commonly known as Vikings—ravaged Northumbria and shook the very existence of its culture.

The high standard attained in learning and the arts was reflected in crafts and industries. We have seen that a leading ecclesiastic such as Eosterwini (Benedict Biscop's cousin, and his successor as an abbot) was devoted to the smith's craft and excelled in it. The impetus extended to far more detailed work, such as cutlery. In the middle of the eighth century, English—and especially northern English—cutlery attained such a standard of refinement and perfection that specimens of its produce were sent as special gifts abroad. Evidence of this is given in a letter of 764, directed to the Englishman Lullus, Bishop of Mainz and successor of Boniface, the great Englishman who established the German Church under the supremacy of Rome. In this letter Guthberht, abbot of Wearmouth and Jarrow, refers to presents which he had sent in 758 by one of his monks who set out on a pilgrimage to Rome, but died on the way. The sender was anxious to know whether his presents, which consisted of a cloak of otter fur and 20 small knives (*cultellos*), had been delivered. To these previous gifts he adds two palliums, *i.e.*, ecclesiastic vestments of the finest work (*subtilissimi operis*), books, and a bell.[15] With an earlier letter, undated but of *circa* 734–746, the priest Ingalice had sent a gift of four small knives. These are described as being made in a manner customary to the country (*cultellos nostra consuetudine factos*).[16]

The special gifts sent abroad were products in which the country—and especially the north—had excelled since the days of Benedict Biscop, such as embroidered vestments and illuminated books, and amongst these gifts are knives. This is a significant fact, more so as the knives were sent to the Rhineland, where, some centuries before, sword and knife making had attained a high standard. From the partly Romanized districts of the lower and middle Rhine have probably come the famous Nydham swords of the fourth

[10] "*Ferrum malleo domandum.*" *See* J. E. King, "Baedae Opera Historica," vol. 2, p. 410. London, 1930. Eosterwini was appointed abbot in 682, and he died probably in 686. *See also* A. Hamilton, " Bede, his Life, Times and Writings," p. 85. Oxford, 1935.

[11] Victoria County History of Durham, vol. 2, p. 83. London, 1905.

[12] "*Ferrea qui domitans potuit formare metalla . . . Malleus in ferrum peditat stridente carmino.*" *See* "Symeonis Monachi Opera Omnia," (edited by Thomas Arnold), vol. I, pp. 276–7. London, 1882.

[13] F. M. Stenton, " Anglo-Saxon England," p. 177 Oxford, 1943.

[14] J. D. Kendrick, " Anglo-Saxon Art to A.D. 900," pp. 118–9. London, 1938.

[15] P. Jaffé, " Monumenta Moguntina," Epistola 134, pp. 300–1. Berolini, 1866.

[16] P. Jaffé, " Monumenta Moguntina," Epistola 77, pp. 215–6.

century, and under the Frankish empire the manufacture of sword blades was a staple industry in these districts.[17] The quality of the cutlery manufactured in the Rhineland in the fourth century was also of a high standard, as is witnessed by a knife found at Mainz.[18] This knife was ornamented with a series of three stars of yellow bronze surrounded by circles of copper inlaid on the top of the blade. A closely similar knife, a reproduction of which is shown in Fig. 2, was found at Winchester, but the inlaid ornamentation is less perfect, so that imitation is likely. Whilst in the fourth century knives manufactured by Romanized Germans appear superior, in the eighth century the tide had evidently turned completely. Otherwise there would be no point in sending specimens from Northumbria as particular gifts to an honoured friend abroad if knives just as good, if not better, were manufactured in his own country.

The knives sent to Mainz are described as being fashioned in a special manner peculiar to the country. According to Baldwin Brown[19] the Anglo-Saxon knife is very constant in its form, a special characteristic being its straightness, by which it differs from the curved knife of the Bronze Age and of Romano-British times. Such a distinction is, however, too general to mark it as a special characteristic of Anglian or Northumbrian cutlery. More likely the characteristic appearance of the knives sent to Mainz was

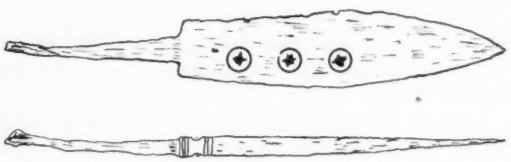


FIG. 2—Knife found at Winchester, showing inlaid ornamentation

caused by ornamentation with inlay, a technique developed in ancient Egypt and perfected in Alexandria, from whence it was also introduced into the West.[20] This technique, when applied to metals, is commonly called "damascening," but it differs from damascening proper, in which strands of metals of different textures and qualities are welded and forged together so that in the end a watery or streaky appearance is obtained.[21] In the finds of the early Middle Ages the technique of damascening proper is confined to the blades of swords. There is no evidence whatsoever of its application to knives. On the other hand there is definite evidence of ornamentation by the technique of inlaying being applied to sword-knives and knives proper. The procedure was that of incrusting the blades with wire of gold, silver, or copper, and we have a very early example of this in the Winchester knife already referred to.

Of sword-knives or scramasaxes, most beautiful specimens have been preserved from the Angl - Saxon period, and amongst the treasures of the British Museum is a sword-knife of about A.D. 800 which is inlaid with Runic lettering and decoration in brass and silver wire ; this knife was found in the Thames.[22] There is also a ninth-century sword-knife, elaborately ornamented by inlay, which was found at Hurbuck farm, near Lanchester (County Durham), and thus very near to Jarrow and Wearmouth ; this knife is part of the biggest hoard of iron weapons and implements discovered in this country. Both these knives are ascribed by Wheeler to the " Hurbuck type " of the eighth, ninth, and tenth centuries, which type has normally deeply scored lines and is often inlaid with copper, brass, or white metal, the back being straight but slightly sloping-off towards the point.[23] As the sword-knife and the knife proper are hardly to be distinguished from one another except by size, we may presume that the knives sent to Mainz between 732 and 758 had the characteristics of the Hurbuck type.

As to the design used with ornamentation by inlay, it may be suggested that the Syrian vine-scroll, which is a motif characteristic to Northumbrian art, was applied. Such inlaid scroll patterns are seen on the blade of a highly ornamented knife found at Sittingbourne in Kent and assigned to the ninth century ; in this knife, which is illustrated in Fig. 3, the maker's name, " Biorhtelm," is also inlaid, with silver letters. The inscription closely corresponds with the Runic inscription on a Northumbrian silver brooch found near Chatham in Kent and now

[17] G. Baldwin Brown, " The Arts in Early England : Saxon Art and Industry in the Pagan Period," p. 213.

[18] E. M. Joyce, *Antiquarian Journal*, 1946, vol. 26, p. 70.

[19] G. Baldwin Brown, " The Arts in Early England," p. 227.

[20] G. Baldwin Brown, " The Arts in Early England," p. 442.

[21] Originally an Indian process which may be traced back many centuries before the Christian era, and described by N. Belaiew, *Journal of the Iron and Steel Institute*, 1918, No. I, p. 433.

[22] British Museum Guide, p. 96. The author is much indebted to Mr. R. Bruce-Mitford, Assistant Keeper of the Department of British and Medieval Antiquities, for showing him the knives in the British Museum, despite the tremendous work caused by re-organization after evacuation.

[23] R. E. M. Wheeler, " London and the Saxons," pp. 178–9. London 1935.

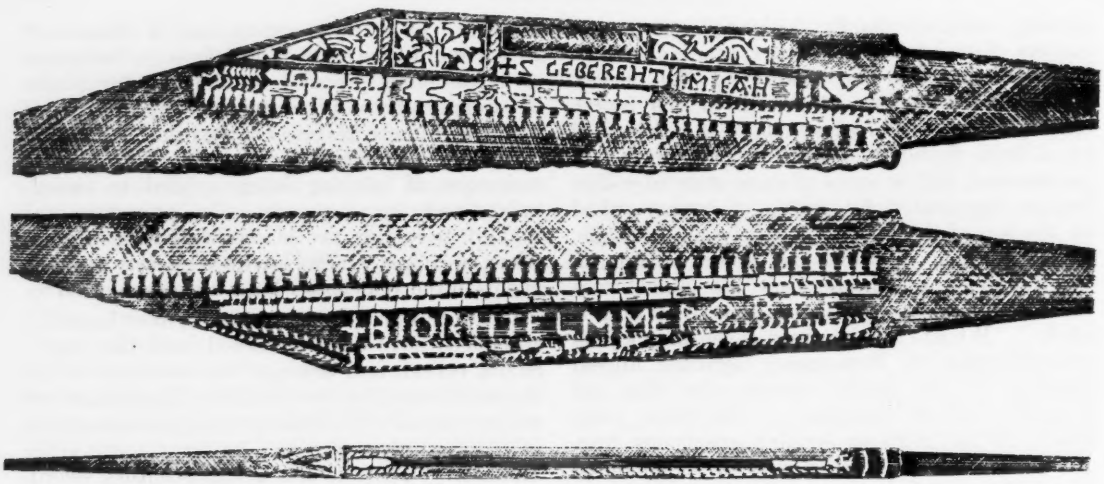


FIG. 3—Ninth-century knife found at Sittingbourne; the maker's and owner's names are inlaid with silver

in the British Museum, and also with inscriptions on a sun-dial on the wall of Kirkdale Church, near Kirby Moorside (Yorkshire), and on the shaft of a cross found at Alnmouth, Northumberland.[24] Thus it may be inferred that the Sittingbourne knife was of Northumbrian manufacture. These three specimens may give an idea of the shape and ornamentation characteristic to the knives sent to Mainz which justified the sender to use the term "*nostra consuetudine fàctos*," already mentioned.

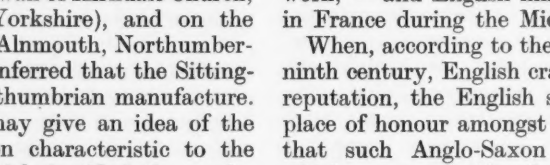
The attainments of the Anglo-Saxon renaissance gave the country a position in arts and crafts the radiance of which lasted far beyond the eighth century. English embroidery, for example, held such a prominent place in the artistic production of the early medieval world that, as late as the time of the Norman conquest, products were exported to Italy under the name of "English work,"[25] and English knives were of high repute in France during the Middle Ages.[26]

When, according to the *Liber Pontificalis* of the ninth century, English craftsmen had a European reputation, the English smith and cutler held a place of honour amongst them. It is remarkable that such Anglo-Saxon cutlery, specimens of which were considered worthy of being sent abroad as special gifts, was manufactured in the north of England, in a district not very far from Sheffield, the cutlery of which has attained world renown.

[24] J. Evans, *Archaeologia*, 1873, vol. 44, pp. 331–4.

[25] H. W. C. Davies, "Medieval England," p. 577. Oxford, 1924.

[26] *See* C. Pagé, "La coutellerie depuis l'origine jusqu'a nos jours," vol. 1, p. 158. Chatellerault, 1898.



Behaviour of a Chromium Steel in the Jominy Hardenability Test*

By W. I. Pumphrey, M.Sc.†

SYNOPSIS

A metallurgical examination, including examination by the electron microscope, has been made of the structures developed along the length of a Jominy bar of a steel possessing unusual hardenability characteristics at certain slow rates of cooling. It has been shown that the structures can be explained on the basis of a derived **S***-curve which is not abnormal in type.*

In a previous investigation[1] of the hardenability characteristics of a series of steels, by means of the end-quench test, it was noted that the hardenability curve of one particular steel (steel No. 5 of the series) indicated a definite increase, and then a decrease in hardness between 1·2 and 1·6 in. from the quenched end of the normal Jominy bar; *i.e.*, the hardenability curve had a definite 'hump' along its tail portion. The same phenomenon had been noticed earlier in a different steel by Williams,[2] who attributed the increase in hardness over a localized section of the tail of the Jominy curve, to the presence in the steel of intermediate transformation products of the type described by Griffiths, Pfeil, and Allen.[3] Williams was, however, unable to show definitely that the 'hump' was due to the presence of such transformation products.

Steel No. 5, the steel noted in the previous investigation, had the analysis given below, and the irregularity along the tail of the hardenability curve was found to occur both with bars of this steel given the normal Jominy quench, and with bars given interrupted end-quenches.

Analysis of Steel No. 5

C, %	Si, %	Mn, %	Ni, %	Cr, %	Mo, %
0·83	0·28	0·82	0·34	1·13	0·05

The interrupted end-quench technique[1] is a method which enables the temperature range, over which transformation takes place in a steel on continuous cooling, to be determined. In this method a number of Jominy bars are made from the steel in question, heated to the appropriate quenching temperature in the usual manner, end-quenched for varying periods of time in the standard Jominy quenching fixture, and then totally quenched in iced brine; hardness measurements being taken along the bars after quenching. From a knowledge of the time/temperature curves at points along the length of a Jominy bar, during cooling in the standard end-quench, it is possible to determine the temperature to which a bar has cooled, at any point along its length, when removed from the standard quenching fixture after a known period of end-quenching. This temperature can then be related to the hardness obtained at that point after total quenching (*see* Fig. 13), and in this way an estimation can be made of the condition of the steel and its degree of completeness of transformation at any temperature during continuous cooling.

In the case of the steel under consideration, a most pronounced 'hump' was obtained with the bar quenched 150 sec. after being placed in the fixture—*see* Fig. 1, reproduced from the paper by Pumphrey and Jones.[1] Owing to the uncertainty regarding the nature of the metallographic structures associated with the 'hump,' it was thought to be of interest to carry out a microscopic examination of this bar. The quenched bar was accordingly sectioned longitudinally, polished and etched in a 4% solution of nitric acid in alcohol, and examined microscopically. The results of this examination are shown in Figs. 2–9.

Discussion

The structures in Figs. 2–9 should be explicable on the basis of a suitable **S**-curve. The approximate form of such a curve can be deduced from a consideration of the data given in Table I, and a knowledge of the cooling behaviour at locations along the length of the Jominy bar. The most probable form of the **S**-curve for steel No. 5 is given in Fig. 12, together with time/temperature cooling curves (extrapolated from the data of Russell and Williamson[4]) at several positions along a Jominy bar end-quenched in the normal way. While it is realized that such a treatment is not strictly correct, the time/temperature curves have been superimposed on the isothermal-transformation curve in Fig. 12 for simplification of the discussion. The vertical dotted line in Fig. 12 indicates the instant of total quenching (after 150 sec. end-quenching) of the bar under consideration.

* Received 20th March, 1947.

† National Physical Laboratory, now at the Department of Metallurgy, Birmingham University.

TABLE I—*Data for Bar given 150 sec. Interrupted End-Quench*

Distance from Quenched End of Bar, in.	Temperature at Instant of Total Quench, °C.	Hardness after Total Quench, D.P.H.	Structure after Total Quench	Figure No.
2·0	495	353	Pearlite	9
1·6	440	460	Fine pearlite and martensite	8
1·4	405	723	Some pearlite and some acicular ferrite and martensite	7
1·2	366	631	Some pearlite, and some acicular ferrite and intermediate structure and martensite	6
1·05	331	509	Intermediate structure and martensite	5
0·95	310	545	Intermediate structure and martensite	4
0·8	269	666	Intermediate structure and martensite	3
0·4	154	825	Martensite and trace of intermediate structure ...	2

Considering now Figs. 2–9, and assuming steel No. 5 to have an **S**-curve of the type shown in Fig. 12, it can be seen that the structures of martensite with increasing amounts of intermediate structure, shown in Figs. 2 and 3, are formed progressively as the cooling rate decreases from the rate at 0·4 in. from the quenched end of the bar to that at 0·8 in. It is apparent from the **S**-curve that at locations nearer the end of the Jominy bar than 0·8 in., the structures obtained along the bar given the interrupted quench should be very similar to the structures obtained at the same locations in the bar given the full end-quench. This is in agreement with the similarity in the hardness results up to 0·8 in. from the quenched ends of the two bars, *see* Fig. 1. At the rates of cooling encountered at 0·95 and 1·05 in. from the quenched end of the bar, transformation begins in the intermediate range but is not complete before the bar is totally quenched after 150 sec., giving the final structures of intermediate phase in a martensite matrix, shown in Figs. 4 and 5. In passing, it is of interest to note that the needle-like appearance of the intermediate constituent in Fig. 3 corresponds very closely to the structure described as lower bainite by Jolivet[5] (*see* Fig. 25 of the paper by Jolivet), and that the structures shown in Figs. 4 and 5 are similar to the upper bainites described by him as "aggregates without any well-defined structure which recall pearlite in process of coalescence." At a cooling rate from the austenitic condition corresponding to that found at 1·2 in. from the end of the bar, transformation in steel

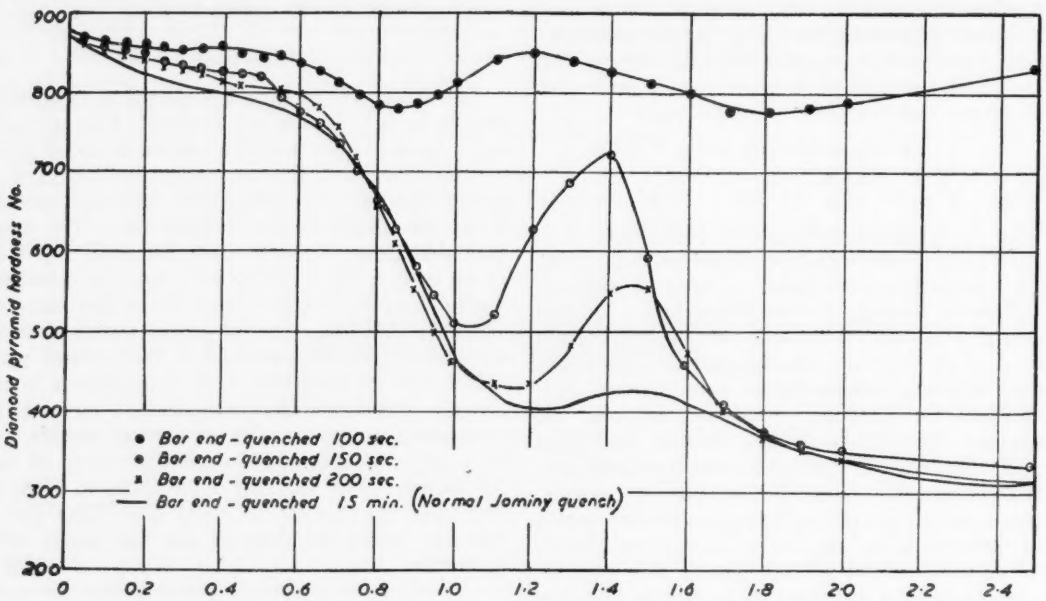


FIG. 1—Hardness results on Jominy bars of steel No. 5 (analysis: C, 0·83%; Si, 0·28%; Mn, 0·82%; Ni, 0·34%; Cr, 1·13%; Mo, 0·05%), brine-quenched after various times in the quenching fixtures, all bars end-quenched from 825° C.

Fig. 2—Microstructure 0·4 in. from quenched end of bar.
Hardness 825 D.P.H.

Fig. 3—Microstructure 0·8 in. from quenched end of bar.
Hardness 666 D.P.H.

Fig. 4—Microstructure 0·95 in. from quenched end of bar.
Hardness 545 D.P.H.

Fig. 5—Microstructure 1·05 in. from quenched end of bar.
Hardness 509 D.P.H.

Fig. 6—Microstructure 1·2 in. from quenched end of bar.
Hardness 631 D.P.H.

Fig. 7—Microstructure 1·4 in. from quenched end of bar.
Hardness 723 D.P.H.

Figs. 2–7—Specimens etched in a 4% solution of nitric acid in alcohol. × 2000

FIG. 8.—Microstructure 1·6 in. from quenched end of bar. Hardness 460 D.P.H. × 2000

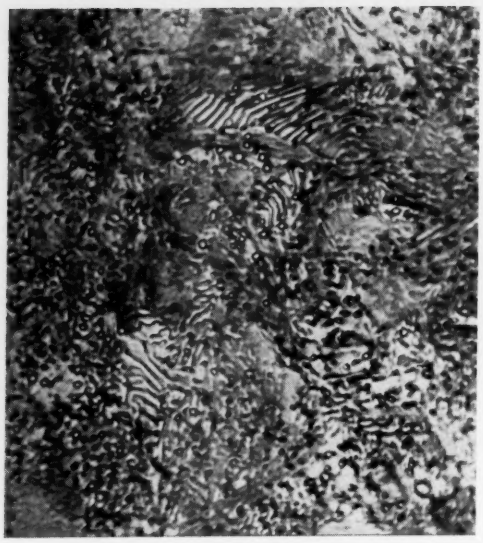


FIG. 9.—Microstructure 2·0 in. from quenched end of bar. Hardness 353 D.P.H. × 2000

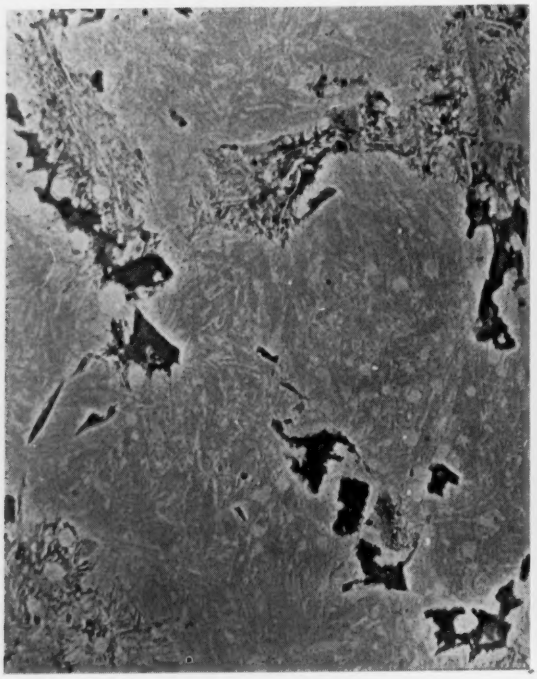


FIG. 10.—Electron micrograph 1·4 in. from the quenched end of the bar. × 4000 (1000 diameters enlarged × 4)

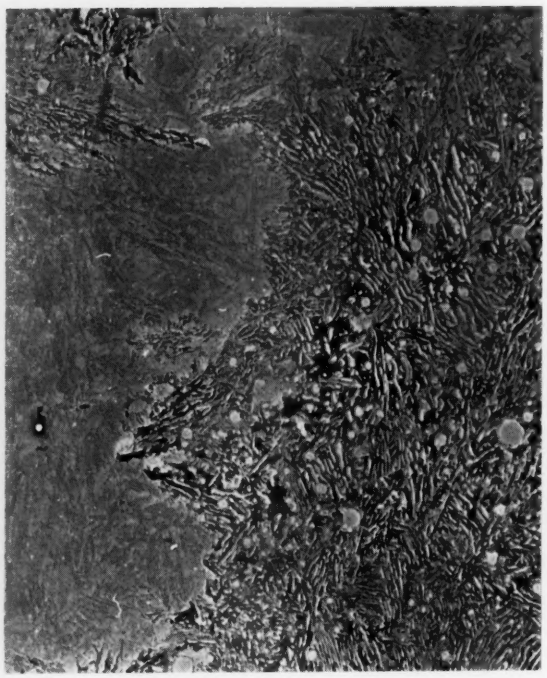


FIG. 11.—Electron micrograph 1·6 in. from the quenched end of the bar. ×4000 (1000 diameters enlarged × 4)

FIGS. 8–11.—Specimens etched in a 4% solution of nitric acid in alcohol

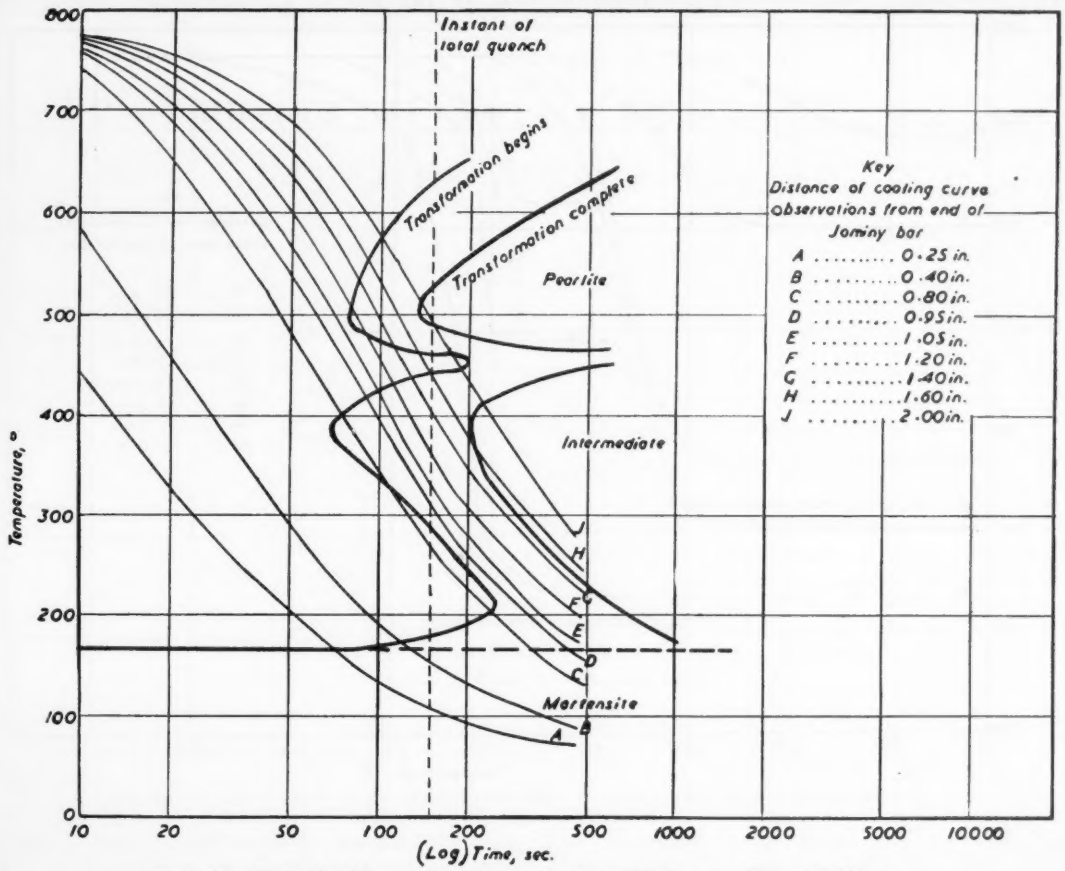


FIG. 12—Probable form of isothermal tranformation curve for steel No. 5

No. 5 commences in the pearlite region (note the dark areas of unresolved pearlite in Fig. 6) although the rate of cooling is too rapid to permit complete transformation to pearlite. Transformation is continued in the intermediate range, but on continuous cooling, the time spent in this range is only sufficient for a slight initial precipitation of acicular ferrite and the formation of a small amount of intermediate product before the residual untransformed austenite is transformed completely to martensite in the total quench after 150 sec. The structure finally consists of acicular ferrite, intermediate structure, and unresolved pearlite, in a martensitic matrix shown in Fig. 6. At the rate of cooling at 1·4 in. from the quenched end, transformation again begins in the pearlite region, with the formation of more pearlite than with the faster rate at 1·2 in. from the end of the bar (compare the size of the dark areas in Figs. 6 and 7). Transformation is again continued in the intermediate range, but the rate of cooling is such that there is only time for a slight initial precipitation of acicular ferrite without the growth of any appreciable amount of intermediate phase, before the total quench after 150 sec. transforms the untransformed austenite to martensite, giving a structure of acicular ferrite and unresolved pearlite in a martensitic matrix (Fig. 7). With lower rates of cooling, such as obtained at 1·6 and 2·0 in. from the quenched end of the bar, pearlitic products of the type shown in Figs. 8 and 9 are obtained, the presence or absence of martensite in the structure depending on the degree of completeness of transformation before the total quench after 150 sec.

Confirmation that the dark areas in Figs. 7 and 8 unresolved by means of the metallurgical microscope were in fact areas of pearlite, was afforded by the results of an examination under the electron microscope of areas 1·4 and 1·6 in. from the quenched end of the bar. The results of this examination are illustrated in Figs. 10 and 11, which clearly reveal the laminated nature of the dark areas. The electron micrographs were obtained from negative replicas made with the resin chosen by Schaefer and Harker[6]; namely, Formvar (polyvinyl formal).

The structures illustrated in Figs. 2–9 are an

XUM

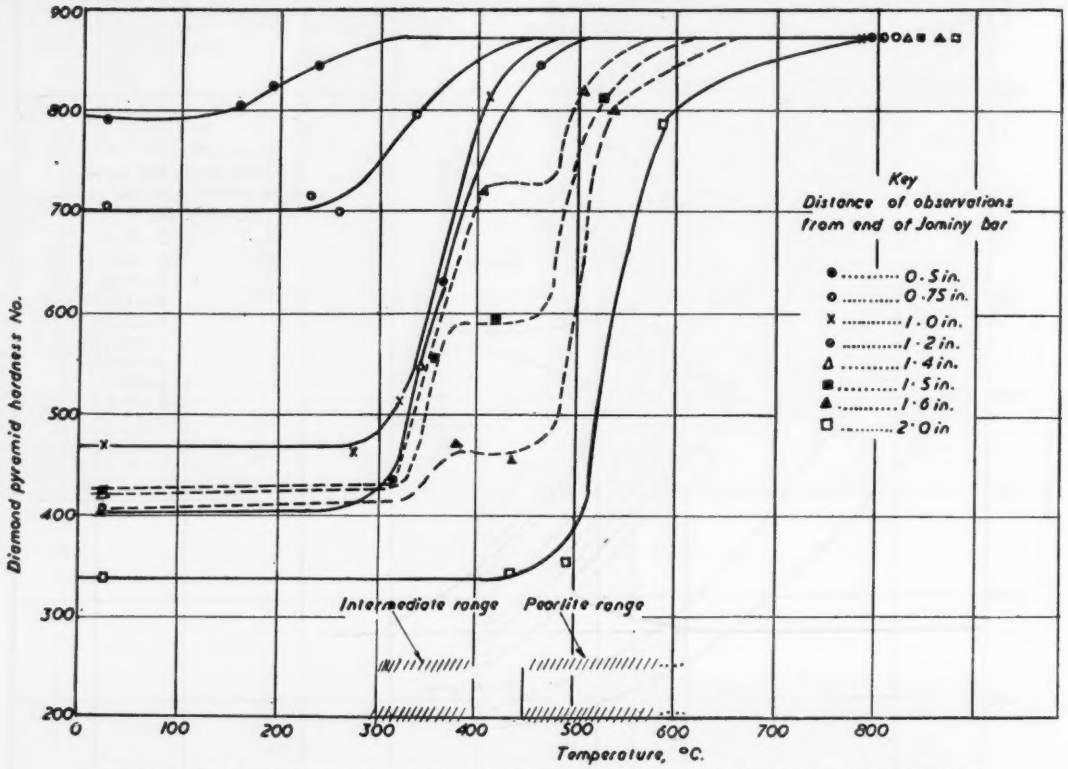


FIG. 13—Hardness/temperature curves at different positions along Jominy bar of steel No. 5

indication of the hardness of the steel, and correspond with the variation in hardness over the tail of the Jominy curve of the bar given the 150-sec. interrupted quench.

The above explanation may be extended to the case of the bar given the normal Jominy quench, using a suggestion from the work of Wever and Mathieu.[7] They showed that if high manganese steels are allowed to transform partially at one temperature T_x, and are then cooled and allowed to transform at a lower temperature T_y, the rate of transformation at temperature T_y is less than if the steels are allowed to transform wholly at the lower temperature without prior higher temperature transformation. For the steel given the normal Jominy quench, the hardness decreases up to 1·05 in. from the quenched end of the bar, as would be expected from Fig. 12. At 1·2 in., the slight initial transformation in the pearlite range retards the rate of transformation in the intermediate range, causing transformation to be incomplete on continuous cooling at a temperature at which any untransformed austenite can transform to martensite. This effect is intensified at 1·4 in.

Assuming the validity of the above argument, it is probable that the curves of Fig. 36 in the paper by Pumphrey and Jones,[1] should be redrawn to show a 'duplex' (*i.e.*, a higher and a lower temperature) transformation at points 1·4, 1·5, and 1·6 in. from the quenched end of the bar, as in Fig. 13 of the present paper. From Fig. 13 it can be estimated that with steel No. 5 the pearlitic range extends down to 450° C. and the intermediate range lies between 400° and 300° C.

References

1. W. I. PUMPHREY and F. W. JONES: *Iron and Steel Institute Spec. Rep. No.* 36, 1946, pp. 47–56.
2. G. T. WILLIAMS: *Transactions of the American Society of Metals*, 1940, vol. 28, p. 157.
3. W. T. GRIFFITHS, L. B. PFEIL, and N. P. ALLEN: *Iron and Steel Institute Spec. Rep. No.* 24, 1939, p. 343.
4. T. F. RUSSELL and J. C. WILLIAMSON: *Iron and Steel Institute Spec. Rep. No.* 36, 1946, pp. 34–46.
5. H. JOLIVET: *Journal of the Iron and Steel Institute*, 1939, No. II, p. 95P.
6. V. J. SCHAEFER and D. HARKER: *Journal of Applied Physics*, 1942, vol. 13, p. 427.
7. F. WEVER and K. MATHIEU: *Mitt. K. W. Inst. Eisenforsch.*, 1940, vol. 22, No. 2, pp. 9–18.

Acknowledgments

The work described above has been carried out as part of the Research Programme of the National Physical Laboratory, and is published by permission of the Director. The author desires to acknowledge the assistance rendered by Mr. J. Trotter, who prepared the electron micrographs.

Deformation of Metals During Single and Repeated Tensile Impact*

By J. A. Pope, Ph.D., Wh.Sch.†

SYNOPSIS

The main purpose of the research was to examine in some detail the process of deformation during single and repeated tensile impact. The falling-tup type of machine was used and an adjustable stop was fitted to the machine so that either a single impact could be split up into a number of stages or repeated impact tests could be carried out with constant velocity of deformation. The mode and spread of deformation was examined by microscopic examination, by Vickers hardness tests, and by measuring the change in diameter of the specimen at various points. The metals subjected to these tests were Lowmoor iron and mild steel. A general theory expressed in dimensionless groups is developed in the early part of the paper to explain the mode of deformation. This gives good agreement with experimental results. It was found that the spread of deformation along the specimen during impact was fundamentally different from that for static deformation. Hardness tests taken along the centre and edge of deformed steel and iron specimens showed that the increase in hardness at the centre of the specimen was larger than that at the edge by a greater amount in impact than in static tests. Tests on brass specimens showed no such hardness variation. Finally it was found that impact-percentage-elongation/static-percentage-elongation is a function of the gauge length of the specimen and is not a fundamental property of the metal.

INTRODUCTION

UP to the present it has been accepted on *prima facie* evidence that the modes of deformation in static and impact tensile tests are identical. In previous work dealing with tensile impact this assumption has been rarely stated although obviously assumed, either by the manner in which the test results are given, or by the actual object of the investigation. For example, the expressing of the rate of straining as inches per second per inch of gauge length obviously assumes that the rate of deformation is uniform along the specimen, and even the comparison of percentage elongations during impact and static tests assumes that the modes of deformation are similar, if such comparison is stated as a fundamental property of the metal, unrelated to the proportions of the specimen. Those who have endeavoured to calculate impact energies from static tests have naturally assumed similar modes of deformation,[1, 2] although not in all cases has this assumption been acknowledged.

The assumption is based on the evidence that a fractured tensile impact specimen has a form similar to a fractured static specimen. This is not true in all cases. Under certain conditions of impact "double necking" (two necks forming on the specimen simultaneously) has occurred. In America this difficulty has been avoided by using a specimen with a slight taper towards the centre of the gauge length.[3]

When one turns to repeated tensile impact, then, not only does double necking become more troublesome, but, when double necking is apparently absent, the ductility of the metal is a function of the energy per blow. This has been demonstrated by Smith and Warnock,[4] Warnock and Brennan,[5] and by Warnock and the present author.[6] In addition, the author found in his investigation that, for mild steel, during repeated impact, the condition of "plastic yield" persisted for longer extensions than in static tests. From previous work the author felt that there was a clear indication that the mode of deformation during repeated impact was different from that in a static test. The purpose of the present investigation was to examine the process of deformation during both single and repeated tensile impact.

The experimental work was carried out before any definite theory was conceived, and it was

* Received 24th March, 1947.

† University of Sheffield.

some months later that a satisfactory theory was evolved to explain the rather bewildering, and apparently conflicting, phenomena observed experimentally. In this paper, however, the mathematical theory is first developed and then various phenomena, observed experimentally, are reported and examined in the light of this theory.

Theory of Deformation during Tensile Impact

The following is a list of symbols used throughout the paper :

Stresses and Elastic Constants (pounds per square inch)

- σ = Stress propagated by the hammer at any time t
- σ_y = Dynamic yield stress for a given condition of impact
- σ_o = Magnitude of initial stress wave, assuming yield does not take place
- E = Young's modulus

Weights (pounds) and Densities (pounds per cubic inch)

- W = Weight of tup (or hammer)
- w = Weight of gauge length of specimen
- ρ = Density of specimen
- W_H = Weight of falling head

Velocities (inches per second)

- V_o = Initial velocity of impact
- V = Velocity of hammer at any time t
- C_o = Velocity of sound in the metal under elastic condition
- V_y = Velocity corresponding to the yield stress
- C = Velocity of propagation of plastic wave at strain ξ

Time (seconds)

- t = Any time measured from the instant of impact
- T = Time at which any given reflection occurs
- $t' = t \times \frac{Pg}{WV_y}$ (*dimensionless*)
- t_n = Time measured from the instant that the velocity of the hammer is V_y/n (*e.g.*, t_2 is measured from when the velocity of the hammer is $V_y/2$)

Miscellaneous

- A = Cross-sectional area of specimen (square inches)
- L = Length of specimen (inches)
- ξ_o = Strain corresponding to yield point
- ξ = Strain
- P = Total force in specimen (pounds) ($= A\sigma y$ if the stress–strain curve is horizontal beyond the yield point)
- S = Displacement of hammer at time t
- $S' = S \times \frac{Pg}{WV_y^2}$
- H = Height of fall of falling head and tup (inches).
- h_1 = Rebound of head from anvil (inches), when falling from height H, with no specimen attached
- Q = Energy required to produce fracture by a single impact (inch-pounds)
- e = Percentage elongation

Stress Waves in an Elastic Rod

Prescott[7] has shown that if a bar DE, Fig. 1 (a), is fixed at E and is struck a blow at end D with a hammer of weight W lb., travelling with velocity V_o ft./sec., then a rectangular stress wave will travel along the bar from D to E at the velocity of sound (C_o), where :

$$C_o = \sqrt{\frac{gE}{\rho}} \quad \text{.....(1)}$$

and the magnitude of the stress :

$$\sigma = \frac{V_o E}{C_o} \quad \text{.....(2)}$$

The propagation of the stress wave given by equation (2) at the speed given by equation (1) is necessary to permit the weight W to move at constant velocity V_o. Similar consideration shows that, at a fixed end, the wave must be reflected with the same sign, thus doubling the stress on reflection, Fig. 1 (b).

In the case shown in Figs. 1 (a) and 1 (b) both ends may be considered fixed, and therefore positive reflection takes place at both ends, the stress rising in jumps of σ at every reflection until eventually the yield stress is exceeded and the

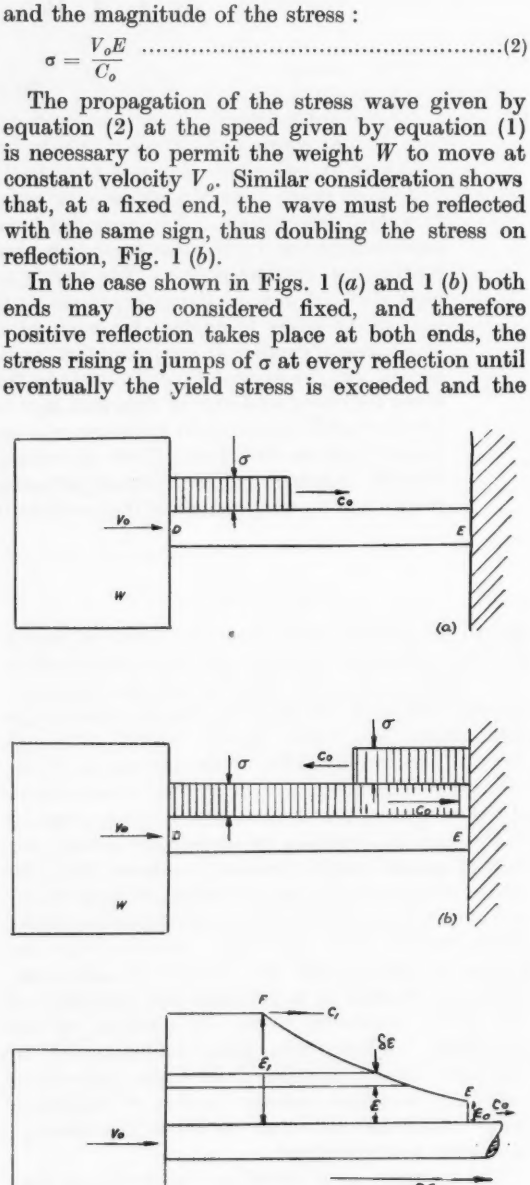


Fig. 1—Plastic strain wave in infinite bar

movement of the hammer is facilitated by plastic deformation.

Bar Struck by a Finite Weight

In equations (1) and (2) the velocity has been assumed to remain constant during the impact. In the case of a finite weight impacting the end of a bar, this is not so. The velocity of the weight will be gradually reduced as its kinetic energy is given to the bar, and therefore the magnitude of the stress wave propagated will be accordingly reduced. It may be shown[8] that for the time interval $0 < t < 2L/C_o$ the stress propagated by the hammer is given by :

$$\sigma = \sigma_o e^{-\frac{A}{W}\sqrt{E\sigma g}t} \quad \ldots\ldots(3)$$

and if the yield stress is not exceeded and w/W is $< 0{\cdot}2$ the maximum stress induced in the specimen may be accurately expressed as :

$$\sigma_{max.} = \sigma_o(\sqrt{W/w} + 1) \quad \ldots\ldots(4)$$

So far we have considered only the compression waves in a bar impacted by a hammer. Relationships (1) to (4) apply equally well to the falling-tup type of machine. In this case the induced stress is tensile and the head end of the specimen (*i.e.*, the end which is brought to rest by the anvil) is equivalent to the hammer end in Fig. 1.

Stresses and Deformation due to Impact when the Yield Stress is Exceeded

All the preceding formulæ are based on the assumption that the stress/strain ratio is independent of the strain. With metal this is only true within the elastic range, and therefore, should a metal be stressed beyond the elastic limit, these formulæ are not applicable.

From equation (1) C_o, the velocity of stress propagation in an elastic rod, equals $\sqrt{gE/\rho}$. Now, for this condition, E represents the slope of the stress–strain diagram. It seems logical then to assume that when the yield stress has been exceeded and the slope of the stress–strain diagram is changing with the value of the strain (*i.e.*, $d\sigma/d\xi = f(\xi)$) that the velocity of propagation of a given part of a plastic wave will depend only upon the strain at that part of the wave.

Consider an infinite bar impacted at one end with a velocity V_o such that the yield stress is exceeded, and at any interval of time t let the strain wave along the bar be represented by Fig. 1 (*c*). The front of the wave E will be the elastic strain ξ_o, corresponding to the strain at yield which is travelling at velocity C_o, and the end of the front F will be some plastic strain ξ_1, travelling at a velocity $C_1 (C_1 < C_o)$. Thus the shape of the wave front will be continually changing with time since E is travelling more rapidly along the bar than F.

Consider the displacement, velocity, and stress due to an element of the wave between strain ξ and $\xi + \delta\xi$. Let the velocity of propagation at strain ξ equal c. Then :

$$\delta S_\xi = \delta_\xi ct$$

$$\delta V_\xi = \frac{\delta S_\xi}{t} = c\delta_\xi \quad \ldots\ldots(5)$$

where δS_ξ = displacement of end of rod due to element, and

δV_ξ = velocity of end of rod due to element.

The increase in strain $\delta\xi$ will be accompanied by a rise in stress $\delta\sigma$, and by considering the equations of momentum the relationship between c and $\delta\sigma/\delta\xi$ can be obtained in exactly the same way as for elastic strain. Then :

Force = Rate of change of momentum

$$A\delta\sigma = \frac{A\rho c\,\delta V_\xi}{g} \quad \ldots\ldots(6)$$

Substituting (5) in (6) gives :

$$c = \sqrt{\frac{\frac{\delta\sigma}{\delta\xi}}{\frac{g}{\rho}}} \quad \ldots\ldots(7)$$

Equation (7) is the same as equation (1) if E is substituted for $\delta\sigma/\delta\xi$ which, of course, is the definition of elasticity.

Substituting (7) in (5) gives :

$$\delta V_\xi = \sqrt{\frac{g}{\rho}\frac{\delta\sigma}{\delta\xi}} \times \delta\xi$$

$$\therefore V_o = \int_o^{\xi_1} \sqrt{\frac{g}{\rho}\left(\frac{\delta\sigma}{\delta\xi}\right)}\,\delta\xi \quad \ldots\ldots(8)$$

Equation (8) fixes the value of ξ_1. The velocity of the bar at any point X, at an instant of time t, where the strain is ξ is :

$$V = \int_o^{\xi} \sqrt{\frac{g}{\rho}\left(\frac{\delta\sigma}{\delta\xi}\right)}\,\delta\xi \quad \ldots\ldots(9)^*$$

Equations (8) and (9) can generally only be solved graphically, as in most cases the shape of the stress–strain curve cannot be expressed by a simple mathematical relationship. The shape of the load–extension diagram for any metal during impact is not accurately known, but for normalized plain carbon steel it is practically a horizontal straight line after the yield point has been exceeded (*see* the results of Brown and Vincent,[9] Clark and Datwyler[10] and Warnock and Pope[6]). In this case $\delta\sigma/\delta\xi = 0$ for all values of stress and strain beyond the yield point, and the velocity of propagation will be zero. This means that the deformation will remain isolated at the end of the bar if infinite in length, or in the case of a tensile specimen of finite length at the two ends. The deformation at the second end is due to the reflection of the elastic wave.

* This solution is similar to that obtained by G. I. Taylor[11] and von Kármán.[12]

In the analysis given below the following assumptions are made :

(1) The load on the specimen remains constant after the metal has yielded, the nominal stress in the specimen remaining constant throughout the plastic deformation and equal to the dynamic yield stress corresponding to the velocity of impact being considered.

(2) The reflection at the tup and head ends of the specimen is perfect.

(3) The weight of the specimen is small compared with the weight of the tup so that the decrease in the magnitude of the elastic wave, while travelling the length of the specimen, is so small that the magnitude of the wave may be considered constant. The decrease is equal to :

$$\sigma_0 e^{-\frac{A}{W}\sqrt{E\rho g}T} \quad \left(1 - \sigma_y e^{w/W}\right)$$

(4) The kinetic and elastic energies of the specimen are so small compared with the plastic energy absorbed, that they may be neglected.

(5) It follows from (3) and (4) that it is assumed that any reduction in the velocity of the tup (or hammer), and therefore reduction in the magnitude of the stress wave, is due only to the energy absorbed by plastic deformation.

Throughout the analysis the hammer type of tensile impact will be considered, as the arguments can be presented rather more simply for this type of machine than for the falling-tup type. Any conclusions drawn for the former type apply equally well to the latter since the falling-tup type of impact can be transferred into an equivalent hammer type by giving the whole system a negative velocity equal to the velocity of the tup.

Case I—The Velocity of Impact (V_o) is so Great that the Initial Stress (σ_o) is Greater than the Dynamic Yield Stress (σ_y)

During the very first instant of impact an element at the hammer end of the bar will have obtained a velocity V_y such that the stress induced in this element equals the yield stress. The right-hand end of this element will continue to travel with velocity V_y (Fig. 2) while the left-hand end attains the velocity of the hammer. Thus the small element is being plastically deformed at the rate of $(V_o - V_y)$ in./sec. The energy of the hammer, however, is being gradually absorbed by the plastic deformation of the element and the rate of deformation is gradually diminished until eventually the hammer speed has been reduced to V_y, when deformation ceases—the time taken for the velocity to fall from V_o to V_y being $(V_o - V_y)W/Pg$ sec.

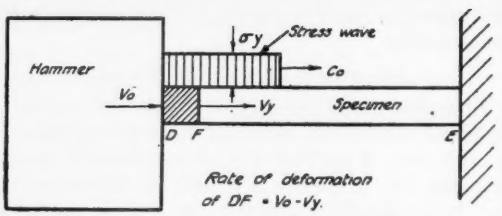


Fig. 2—Deformation at initial stage of impact ($V_o > V_y$, $t < C_o/l$)

During this time the right-hand end of the element has been travelling with velocity V_y and therefore propagating a stress wave to the right at velocity C_o and magnitude σ_y.

If the metal remains perfectly plastic throughout the whole deformation, then the deformation at the hammer end will remain isolated ; the distance which the deformation will spread along the specimen being the minimum compatible with the changes of shape and orientation of the crystals involved. Should, however, the metal not yield plastically, but gradually work-harden during deformation, then the deformation will not remain isolated but spread gradually along the specimen, as shown by equations (7), (8), and (9).

Distribution of Energy to the Specimen and the Effect of Reflection from the Fixed Ends

While the portion DF is being deformed by the initial stage of the impact, a stress wave of magnitude σ_y is travelling with velocity C_o towards E (Fig. 2). It arrives at E at a time L/C_o sec., zero time being taken at the instant of impact. This wave is propagated throughout the whole time the velocity of the hammer is falling from V_o to V_y, *i.e.*, $(V_o - V_y)W/Pg$ sec., which is greater than L/C_o. Thus the stress wave will arrive at E long before the deformation at DF has ceased. When the wave σ_y has reached the fixed end E, the portion of the specimen FE is travelling with velocity V_y, and, under elastic condition, the stress wave would be reflected at E, giving a total stress equal to $2\sigma_y$, in order to facilitate the plane F to continue moving with velocity V_y. This, however, is impossible, since

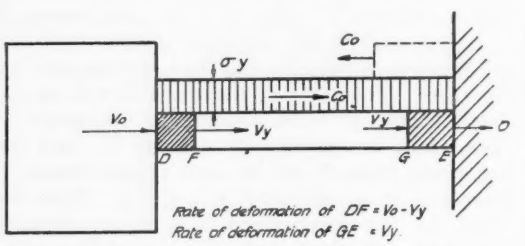


Fig. 3—Deformation at both ends of the specimen simultaneously ($V_o > V_y$, $t > C_o/l$)

σ_y is the maximum stress the specimen can support. Therefore, plastic deformation will take place in the portion GE at a rate equal to V_y (Fig. 3). Eventually deformation at end D will cease and both hammer and portion DG of the specimen will then have velocity V_y. The velocity of the hammer will be gradually reduced as the plastic deformation at GE absorbs its energy. The velocity of plane G at time t will be equal to the velocity of the hammer at time :

$$\left(t - \frac{L}{C_o}\right)$$

Let the velocity of G at time t be V ; then :

$$V = V_o - \frac{Pg}{W}\left(t - \frac{L}{C_o}\right)$$

where Pg/W = deceleration of the hammer.

Let the velocity of G due to the first wave at any given time be reduced to V, and the magnitude of the stress wave to σ ; $(\sigma = VE/C_o)$ where $V < V_y$ and $\sigma < \sigma_y$. A portion of this wave, equal to $(\sigma_y - \sigma)$ can now be reflected back from G to F with velocity C_o (Fig. 4). Therefore the plane G now has a velocity due to the combined effect of the incident stress wave σ and the reflected stress wave $\sigma_y - \sigma$. The incident stress wave gives G a velocity of V from left to right, while the reflected wave gives G a velocity from right to left equal to the rate of elastic strain it produces in the portion GF of the specimen, namely :

$$(\sigma_y - \sigma)C_o/E = V_y - V$$

Hence, resultant velocity of G :

$$= V - (V_y - V) = 2V - V_y \quad \text{.....(10)}$$

which, of course, is also the rate of deformation of the portion GE.

It follows from equation (10) that the deformation will cease in portion GE when the velocity of plane G has been reduced to $\frac{1}{2}V_y$ and the velocity of the hammer to :

$$\tfrac{1}{2}V_y - \frac{L}{C_o} \cdot \frac{Pg}{W}$$

The deformation at GE commences at time :

$$L/C_o \quad \text{.....(11}a\text{)}$$

propagation at elastic wave from G commences at time :

$$(V_o - V_y)\frac{W}{Pg} + \frac{L}{C_o} \quad \text{.....(11}b\text{)}$$

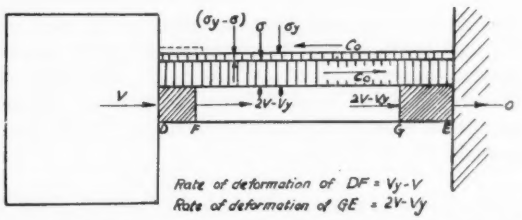


FIG. 4—Reflection at hammer end, first stage ($V < V_y$)

and deformation at GE ceases at time :

$$(V_o - \tfrac{1}{2}V_y)\frac{W}{Pg} + \frac{L}{C_o} \quad \text{.....(11}c\text{)}$$

Thus deformation can take place at both ends of the specimen simultaneously.

Before considering any further reflections the deformation and energies absorbed at each end during the first stage of the impact will be considered, *i.e.*, the time the velocity of the hammer is being reduced from V_o to :

$$V_y - \frac{L}{C_o} \cdot \frac{Pg}{W}$$

which is from $t = 0$ to

$$t = V_o - V_y\left(\frac{W}{Pg} + \frac{L}{C_o}\right)$$

Thus the total deformation at DF will be the deformation which takes place at DF while the velocity of the hammer is being reduced from V_o to V_y, *i.e.* :

Deformation at DF = Movement of hammer (S_o) − Movement of plane $F(S_F)$

The deceleration of the hammer is equal to Pg/W, where P is the load on the specimen $(= A\sigma_y)$ which, by assumption (1), is constant. Then :

$$S_o = \left(\frac{V_o^2 - V_y^2}{2Pg}\right)W$$

and :

$$S_F = V_y(V_o - V_y)\frac{W}{Pg}$$

$$\text{Deformation at } DF = (V_o - V_y)^2\frac{W}{2Pg} \quad \text{.....(12)}$$

Energy absorbed at $DF = P \times$ deformation =

$$(V_o - V_y)^2\frac{W}{2g} \quad \text{.....(13)}$$

Portion GE—The deformation of GE equals the movement of the plane G. The velocity of G remains constant at V_y from time L/C_o until time :

$$\frac{(V_o - V_y)\ W}{Pg} + \frac{L}{C_o}$$

$$\therefore \text{Deformation} = V_y(V_o - V_y)\frac{W}{Pg} \quad \text{.....(14)}$$

$$\text{Energy absorbed} = V_y(V_o - V_y)\frac{W}{g} \quad \text{.....(15)}$$

The ratio of the energy absorbed at DF to that at GE is of course :

$$\frac{\text{Equation (13)}}{\text{Equation (15)}} = \frac{V_o - V_y}{2V_y} \quad \text{.....(16)}$$

If $V_o = kV_y$, then :

$$\frac{\text{Equation (13)}}{\text{Equation (15)}} = \frac{(k-1)}{2} \quad \text{.....(17)}$$

Therefore if V_o is $> 3V_y$ then during the first stage of the impact more deformation will take place at the hammer end (upper end with tup machine) than at the fixed end (lower end with tup machine). If, however, $V_y < V_o < 3V_y$ then

the greater deformation will be at the fixed end (or at the tup) during this stage.

If, as a cross-check, the energies given by equations (13) and (15) be compared with the total loss of kinetic energy by the hammer, it will be found that :

Energy absorbed by plastic deformation ((13) + (15))	+	Elastic and kinetic energy in the specimen.	=	Loss of kinetic energy of the hammer	...(18)

which, of course, is correct. It will be seen from equation (18) that in this case the effect of allowing for the time taken for the stress wave to travel along the specimen is the same, from the point of view of energy, as allowing for the elastic and kinetic energies in the specimen. When the number of stress waves travelling along the specimen increases, then the effect of allowing for the time L/C_o a wave takes to travel along the specimen becomes more complex.

At various times it causes one of the following :

(*a*) Elastic recovery along the centre portion of the gauge length (*i.e.*, that portion of the gauge length which is not affected by the necks which have formed at the ends of the specimen).

(*b*) Uniform plastic deformation along the centre portion of the gauge length.

(*c*) Secondary deformation at the ends of the specimen.

From the energy point of view all are small and may be neglected provided that w/W is small. From the point of view of spread of deformation (*b*) might be of importance. Neglecting the small intervals of time during which (*a*) and (*c*) are produced, uniform plastic deformation occurs along the gauge length while the rate of plastic deformation at the end from which the wave is reflected, is increasing. The rate of uniform deformation increases as the number of waves which are travelling the specimen increases. If the velocity of the hammer is low, so that a number of stress waves are travelling the specimen before the yield stress has been reached, then the rate of uniform deformation rises in magnitude by $(w/W)V_y$ in every time interval L/C_o, until it has reached the magnitude it would have had for this number of reflections, had the initial velocity of impact been greater than V_y. That is, it is sufficiently accurate to assume that the rate of uniform plastic deformation is unaffected by the initial velocity of impact, and depends only upon the number of stress waves which are travelling the specimen.

In the majority of experiments described in this paper the weight of the tup (W) was 15·25 lb., and the weight of the specimen 0·01128 lb., giving a value for w/W equal to 0·00074. This is sufficiently small to justify neglecting the effect of the time taken for the stress wave to travel the length of the specimen and in future an additional assumption will be made, namely, that the velocity of the hammer is reduced so little during the time L/C_o (actually $V_y(w/W)$), that it may be neglected, and therefore all stress waves travelling the specimen will have constant magnitudes while travelling from one end to the other. This has the great advantage that the problem can be analysed using non-dimensional quantities so that graphical representations of a wide range of problems can be simply developed.

Reflection from the Hammer End

It has already been shown that when the magnitude of the first wave has been reduced to σ, where $\sigma < \sigma_y$, the rate of deformation of GE, is :

$$2V - V_y \quad \text{.....(19)}$$

and the magnitude of the reflected wave is :

$$\sigma_y - \sigma = \frac{E}{C_o}(V_y - V) \quad \text{.....(20)}$$

Thus, if the first wave has a magnitude σ at a time t_1 after the velocity of the hammer was V_y, the rate of deformation at GE is :

$$2\left(V_y - \frac{Pg}{W}t_1\right) - V_y \quad \text{.....(21)}$$

Thus deformation at GE will cease after a time of :

$$\frac{V_y W}{2Pg} \text{ sec.} \quad \text{.....(22)}$$

The total extension of GE at any time is :

$$\int_0^{\frac{V_y W}{2Pg}} \left(V_y - 2\frac{Pg}{W}t_1\right) dt_1$$

$$= \left[V_y t_1 - \frac{Pg}{W}t_1^2\right]_0^{\frac{V_y W}{2Pg}} \quad \text{.....(23)}$$

The total extension at GE while the velocity of the hammer is falling from V_y to $\frac{1}{2}V_y$ is :

$$\tfrac{1}{2}\frac{WV_y^2}{2gP} \quad \text{.....(24)}$$

and energy absorbed :

$$= \tfrac{1}{2}\frac{WV_y^2}{2g} \quad \text{.....(25)}$$

During the same period deformation is taking place at the hammer end.

The stress wave $(\sigma_y - \sigma)$ arrives at D at $t_1 = 0$ (L/C_o neglected). Here, under normal elastic conditions, this wave would be reflected, in order to facilitate the velocity V of the hammer. But at the moment the specimen is stressed to its maximum (σ_y) and any reflection will cause plastic deformation of DF (Fig. 4). The rate of this deformation must be equal to the rate of the would-be elastic strain under elastic conditions, namely :

$$\frac{C_o}{E}(\sigma_y - \sigma) = V_y - V$$

No stress can be transmitted on reflection to

the right of F while $V > \frac{1}{2}V_y$, since the stress due to the reflection at E is equal to σ_y.

Rate of deformation at $DF = V_y - V = \frac{Pg}{W}t_1$ ...(26)

Total deformation at DE while the velocity of the hammer is falling from V_y to $\frac{1}{2}V_y = \frac{1}{4}\frac{WV_y^2}{2_yP}$ .....(27)

Energy absorbed $= \frac{1}{4}\frac{WV_y^2}{2g}$ .....(28)

Thus it is seen that again deformation occurs at both ends simultaneously, twice as much energy being absorbed at GE as at DF, while the velocity of the hammer falls from V_y to $\frac{1}{2}V_y$. Also from equations (21) and (26) it can be seen that the rates of deformation are decreasing at GE and increasing at DF with time. When $t_1 = WV_y/2Pg$ (*i.e.*, $V = \frac{1}{2}V_y$) the rate of deformation at GE is zero and that at DF a maximum and equal to $\frac{1}{2}V_y$.

When the velocity of the hammer is less than $\frac{1}{2}V_y$, $2\sigma < \sigma_y$, deformation at GE ceases for an instant. But since the stress in DF is $< \sigma_y$ a portion of the wave reflected at D can be transmitted to the right of F (Fig. 5). The portion of the wave reflected equals $\sigma_y - 2\sigma$, and the rate of plastic deformation in DF is equal to :

$$\frac{C_o}{E}(3\sigma - \sigma_y) = 3V - V_y$$

At any instant of time t_2 measured from the time when $V = \frac{1}{2}V_y$, the velocity of the hammer (V) is given by :

$$V = \tfrac{1}{2}V_y - \frac{Pg}{W}t_2$$

Rate of plastic deformation at DF

$$= \frac{V_y}{2} - \frac{3Pg}{W}t_2 \text{.....(29)}$$

This decreases with time and equals zero when :

$$t_2 = \frac{WV_y}{6Pg} \text{.....(30)}$$

Total plastic deformation at DF

$$= \left[\frac{V_y}{2}t_2 - \frac{3}{2}\frac{Pg}{W}t_2^2\right]_0^{\frac{W}{6}\frac{V_y}{Pg}} = \frac{1}{12}\frac{WV_y^2}{2gP} \text{.....(31)}$$

Energy absorbed $= \frac{1}{12}\frac{WV_y^2}{2g}$ .....(32)

The stress wave $\sigma_y - 2\sigma$ which is reflected from

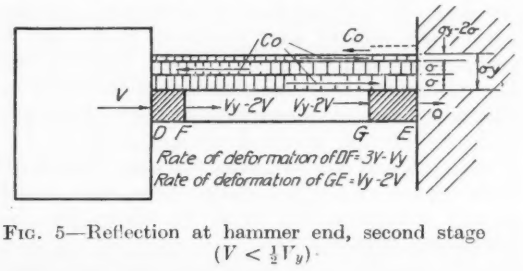


FIG. 5—Reflection at hammer end, second stage ($V < \frac{1}{2}V_y$)

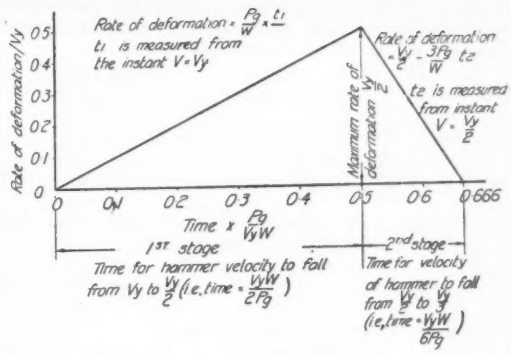


FIG. 6—Deformation at DF during first reflection at the hammer end

F to D is reflected from D where it causes plastic deformation at the rate of :

$$V_y - 2V = V_y - 2\left(\frac{V_y}{2} - \frac{Pg}{W}t_2\right) = \frac{2Pg}{W}t_2 \text{.....(33)}$$

Total plastic deformation at $GD = \frac{Pg}{W}\left[t_2^2\right]_0^{\frac{W}{6}\frac{Vy}{gP}}$

$$= \frac{1}{18}\frac{WV_y^2}{2gP} \text{.....(34)}$$

Energy absorbed $= \frac{1}{18}\frac{WV_y^2}{2g}$ .....(35)

No stress wave can be reflected back from G to F until the velocity of the hammer has fallen to $< \frac{1}{3}V_y$.

Thus reflection at the hammer end (second reflection) is taking place from the time the velocity is V_y until it is $\frac{1}{3}V_y$. From the time the velocity of the hammer is falling from V_y to $\frac{1}{2}V_y$ the rate of plastic deformation is rising from zero to $\frac{1}{2}V_y$, and from the time the velocity of the hammer is falling from $\frac{1}{2}V_y$ to $\frac{1}{3}V_y$ the rate of plastic deformation of DF falls from $\frac{1}{2}V_y$ to zero. The rate of deformation is a linear function of time (equations (26) and (29)), as shown in Fig. 6.

General Solution : Reflection n

During the nth reflection the rate of deformation will be a maximum when the velocity of the hammer is V_y/n and the rate of deformation will then be equal to V_y/n. The reflection will commence when the velocity of the hammer is $V_y/(n-1)$ and cease when its velocity is $V_y/(n+1)$.

Time during which the rate of deformation is increasing :

$$= \frac{1}{n(n-1)}\frac{WV_y}{Pg} \text{.....(36)}$$

Time during which rate of deformation decreases :

$$= \frac{1}{n(n+1)}\frac{WV_y}{Pg} \text{.....(37)}$$

The rate of deformation is a linear function of time and therefore total deformation during the period when the rate of deformation is increasing :

$$= \frac{V_y}{2n} \times \frac{1}{n(n-1)} \frac{WV_y}{Pg} = \frac{1}{n^2(n-1)} \frac{WV_y^2}{2gP} \quad \ldots\ldots(38)$$

Energy absorbed during this stage :

$$= \frac{1}{n^2(n-1)} \frac{WVy^2}{2g} \quad \ldots\ldots(39)$$

Similarly, total deformation during the period when the rate of deformation is decreasing :

$$= \frac{1}{n^2(n+1)} \frac{WV_y^2}{2gP} \quad \ldots\ldots(40)$$

Energy absorbed during this stage :

$$= \frac{1}{n^2(n+1)} \frac{WV_y^2}{2g} \quad \ldots\ldots(41)$$

Total deformation during nth reflection :

$$= \frac{WV_y^2}{2gP} \times \frac{2}{n(n^2-1)} \quad \ldots\ldots(42)$$

Energy absorbed during nth reflection :

$$= \frac{2}{n(n^2-1)} \frac{WV_y^2}{2g} \quad \ldots\ldots(43)$$

By measuring the time from the instant that the velocity of the hammer is V_y, we have :

Time at which deformation commences :

$$= \frac{(n-2)}{(n-1)} \frac{WV_y}{Pg} \quad \ldots\ldots(44)$$

Time at which the rate of deformation is a maximum:

$$= \frac{(n-1)}{n} \frac{WV_y}{Pg} \quad \ldots\ldots(45)$$

Time at which deformation ceases :

$$= \frac{n}{n+1} \frac{WV_y}{Pg} \quad \ldots\ldots(46)$$

Consider, now, the $(n+1)$th reflection. This reflection takes place at the other end of the specimen. From equation (44) this reflection will commence at time :

$$\frac{n-1}{n} \frac{WV_y}{Pg} \quad \ldots\ldots(47)$$

which is the same as equation (45). Thus the $(n+1)$th reflection, which is at the other end of the specimen, commences when the nth reflection is producing the maximum rate of deformation. Similarly the rate of deformation will be a maximum in the $(n+1)$th reflection when :

$$t = \left\{\frac{(n+1)-1}{(n+1)}\right\} \frac{WV_y}{Pg} = \frac{n}{n+1} \frac{WV_y}{Pg} \quad \ldots\ldots(48)$$

This equation is the same as equation (46), thus the $(n+1)$th reflection has the maximum rate of deformation when the nth reflection is about to cease. Consider the $(n+2)$th reflection. This is at the same end of the specimen as the nth reflection. Substituting in equation (44), deformation commences when :

$$t = \frac{n}{n+1} \frac{WV_y}{Pg} \quad \ldots\ldots(49)$$

Thus the $(n+2)$th reflection commences at the instant that the previous reflection at that end (the nth reflection) has ceased. (Actually this is not quite correct as there is an interval of time equal to $2L/C_o$ when deformation ceases completely before the subsequent wave at that end commences.) There is therefore always plastic deformation at each end, but the rate of deformation is continually varying from zero to a maximum, the value of the maximum depending upon the reflection. The maximum rate of deformation decreases as the impact continues.

When the rate of deformation at one end is zero, the deformation at the other end is a maximum, thus facilitating the continuous motion of the hammer.

Figure 7 shows the values for the time intervals, deformations, and energies for the first ten reflections plotted on a base of time. Curves (*a*) and (*b*) show the variation of rate of deformation with time for the hammer end and the fixed end respectively, and curves (*c*) and (*d*) give the energies absorbed at the hammer end and the fixed end respectively. Figure 7 is based on an initial velocity equal to V_y and covers a range of velocities from V_y to 0. If the initial velocity is above V_y then, until the velocity of the hammer is reduced to this value, energies, etc., are best calculated from equations (11) to (17).

In Fig. 7 time is expressed as a fraction of V_yW/Pg rates of deformation as fractions of V_y, and energies in terms of $WV_y^2/2g$. Thus curves (*a*), (*b*), (*c*), and (*d*) are general curves, using dimensionless groups, and therefore can be applied to any tensile impact test within the velocity range V_y to zero.

Case II—The Velocity of Impact is Less than V_y

In the case when the velocity of impact is less than V_y the yield strength of the material may be exceeded only after a number of reflections, and when plastic deformation does commence it will commence at a rate equal to the velocity of the hammer (w/W is assumed to be small).

The straight-line graph (*e*) on Fig. 7, which gives the peaks of the velocity of deformation curves (*a*) and (*b*), represents the total rate of deformation which is equal to the velocity of the hammer.

Thus, given that the velocity of impact is V, where $V/V_y = 0{\cdot}36$, say (point P on Fig. 7), draw through P a horizontal line which cuts curve (*e*) at Q. Through Q draw a vertical line cutting curves (*d*), (*c*), (*b*), and (*a*) at R, S, T, and U respectively and the abcissa at V. This vertical line represents the commencement of deformation. The initial rate of deformation at the hammer end is $V_y \times UV$ and at the fixed end is $V_y \times TV$. Through R and S draw horizontal lines, then these lines represent the horizontal axes from which energy measurements must be taken for this particular impact.

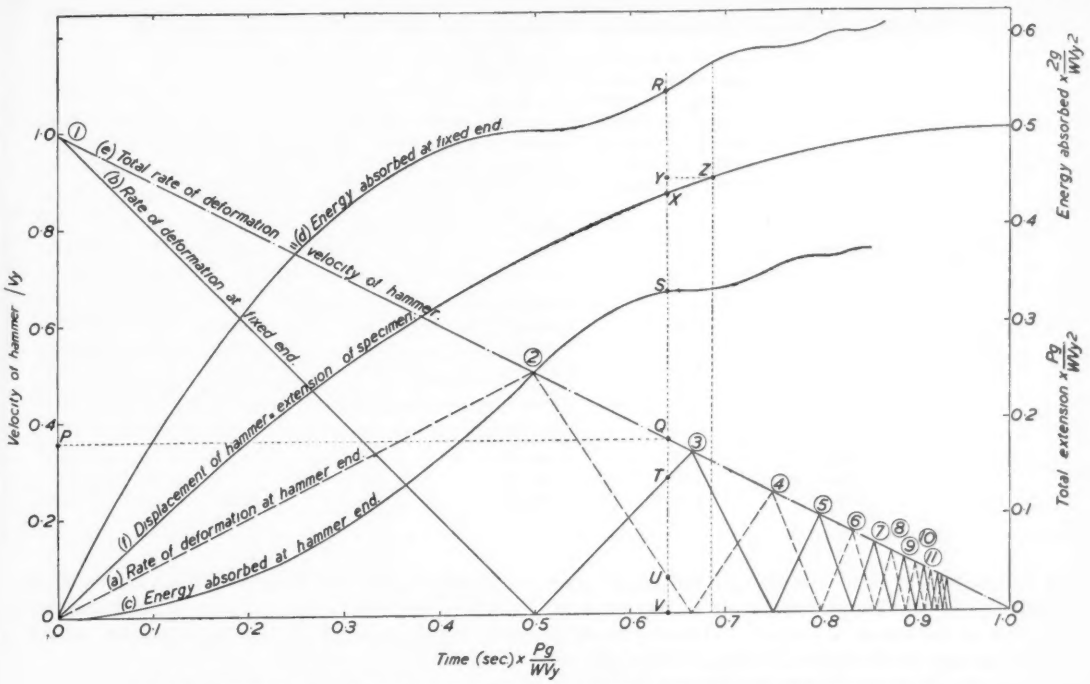


FIG. 7—Diagram of deformation processes during tensile impact, velocity of impact V_y

If it is required to calculate the initial rates of deformation, then, if $(m-1)V < V_y < mV$, m being a whole number, the deformation will commence at the hammer end if m is even, and at the fixed end if it is odd. The initial rate of deformation at the end where deformation is initiated will be equal to $mV - V_y$ and at the other end $(V_y - (m-1)V)$.

Duration of Impact

If the initial velocity of the hammer were V_y, then the displacement in time t, which is also the extension of the specimen, is given by :

$$S = V_y t - \frac{Pg}{2W} t^2 \quad \text{.....(50)}$$

but in Fig. 7 time is expressed as *time* t (*seconds*) $\times Pg/V_yW$. Substituting in equation (50) we have :

$$S = \frac{WV_y^2}{Pg}\left\{t \times \frac{Pg}{V_yW} - \tfrac{1}{2}\left(t\,\frac{Pg}{V_yW}\right)^2\right\}$$

Writing S' for $S \times Pg/WV_y^2$ and t' for $t \times Pg/V_yW$, we get :

$$S' = t' - \tfrac{1}{2}t'^2 \quad \text{.....(51)}$$

Thus in Fig. 7 the displacement curve is plotted using the dimensionless group $S \times Pg/WV_y^2$ for displacement and $t \times Pg/V_yW$ for time. This is curve (f) and is a general curve which is applicable to any impact.

To obtain the duration of the deformation the procedure is as follows : Let the time RV cut curve (f) at X. If the extension of the specimen is x, mark upwards from X a distance XY equal to $x.Pg/WV_y^2$. Through Y draw a horizontal line cutting curve (f) at Z. Then, the distance YZ represents the duration of the deformation. The actual time in seconds equals $YZ \times WV_y/Pg$ or $YZ \times WC_o/EAg$.

Application of Theory to Test Results

The validity of the theory may be checked by applying it to actual test results. If it explains rationally and satisfactorily the rather bewildering and apparently conflicting phenomena which the experiments reveal, then it is, at least, an indication that the dynamic relationships developed are basically correct.

Before any deductions are made, however, the value of σ_y must be obtained for each metal for all velocities. The assumption made so far is that the stress throughout the deformation is constant and equal to the dynamic ultimate stress. Experiment has shown that the dynamic ultimate stress increases with the velocity of impact.

The author, in a previous investigation,[6] has found the dynamic ultimate stress for the mild steel used in these tests with some accuracy, and these values are used for obtaining the various constants in Table I.

Unfortunately, however, no results are available

TABLE I—*Values of Dimensionless Constants for Mild Steel and Lowmoor Iron*

Weight of tup (W) = 15·25 lb.
Area of specimen (A) = 0·0398 sq. in.
Density = 0·283 lb./cu. in.
Young's modulus (E) for mild steel = $29 \cdot 15 \times 10^6$ lb./sq. in.
Young's modulus (E) for Lowmoor iron = $28 \cdot 5 \times 10^6$ lb./sq. in.
Speed of sound (C) for mild steel = $1 \cdot 1995 \times 10^5$ in./sec.
Speed of sound (C) for Lowmoor iron = $1 \cdot 97 \times 10^5$ in./sec.

Velocity of Impact, in./sec.	Yield Stress, σ_y, lb./sq. in.	Velocity, in./sec.	$V_y W/Pg$, sec.	WV_y^2/Pg, in.	$WV_y^2/2g$, in. lb.	V/V_y
			(1) *Lowmoor Iron*			
266	58300	404	0·00685	2·76	3210	0·66
133	48400	334	0·00685	2·29	2200	0·4
66·5	45500	315	0·00685	2·16	1960	0·211
			(2) *Mild Steel*			
266	66300	454	0·00678	3·08	4050	0·587
133	62300	426	0·00678	2·89	3580	0·312
66·5	61000	417·5	0·00678	2·82	3440	0·159

for the Lowmoor iron. Smith and Warnock[4] give details of tensile impact tests on Lowmoor iron for various velocities of impact. Their estimation of the energy to fracture, however, is incorrect, as they omitted to allow for the energy which is supplied by the falling head in the tup type of machine. Their energies have been corrected as near as possible (the exact weight of the falling head was not known in their experiments) by using the following formula suggested to the author by Dr. J. Dick :

$$Q = \frac{W_H \left(1 + \sqrt{\frac{h_1}{H}}\right)^2}{1 + \frac{W}{W_H}}$$

Then, dynamic mean stress during impact :

$$= \frac{Q}{AeL} \qquad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (52)$$

which, from the previous assumptions, must be taken as equal to the dynamic ultimate stress. The values obtained for the dynamic mean stress, with Smith and Warnock's results, are plotted against velocity of impact in Fig. 8. These results, at the best, cannot be guaranteed within 5%. The Lowmoor iron used by Smith and Warnock had a static ultimate stress of 22·7 tons/sq. in., while that used by the author in the present investigation had a static ultimate stress of 20 tons/sq. in. The values taken, in the present tests, for the dynamic ultimate stress for Lowmoor iron, are those given by the broken curve in Fig. 8. This curve is obtained by subtracting 2·7 tons/sq. in. from the full curve on Fig. 8. It cannot be regarded as very satisfactory and will reduce somewhat the value of the tests on the Lowmoor iron.

Table I gives the values of the various dimensionless constants for mild steel and Lowmoor iron corresponding to the appropriate velocities of impact.

Apparatus

The impact testing machine used was of the falling-tup type and has been described fully elsewhere.[4] The upper end of the specimen is fixed to a "falling head," and the lower end attached to the tup (Fig. 9) ; the falling head is raised vertically through a measured distance and then released. The head falls freely until it strikes a large cast-iron anvil which brings it to rest immediately. The tup, however, continues its downward journey thus stressing the specimen. In this type of machine the specimen receives energy both from the falling head (due to rebound of the head from the anvil) and from the tup.

It was desired to be able to split up a single

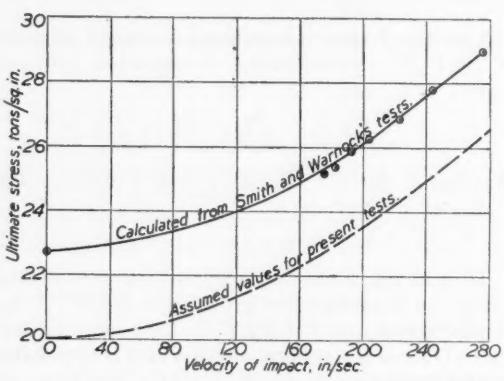


FIG. 8—Effect of velocity of impact upon the ultimate stress of Lowmoor iron

impact into a number of separate stages so that the process of deformation during a single impact could be examined. The method of holding and supporting the specimen in the falling head is shown in Fig. 10.

The specimen is screwed at both ends and at the centre (Fig. 11 (*b*)). The distance between the two upper screwed portions includes the gauge length under test. First the stop is screwed into position and then the adjustable head is screwed on to the top of the specimen. The specimen is next lowered into the falling head from the top, and the tup is screwed on to the lower portion of the specimen, which projects from the falling head. The specimen being held firmly, the adjusting head is now turned until the stop is raised the required distance from the bottom of the hole. It is obvious

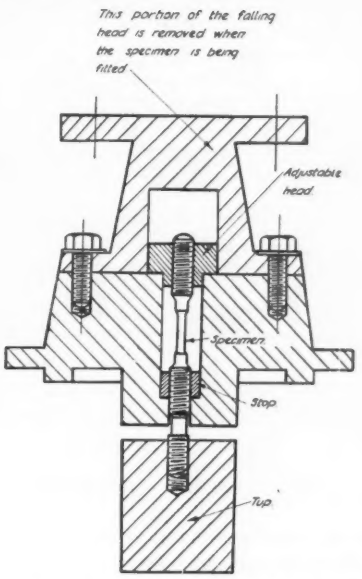


FIG. 10—Arrangement of falling head

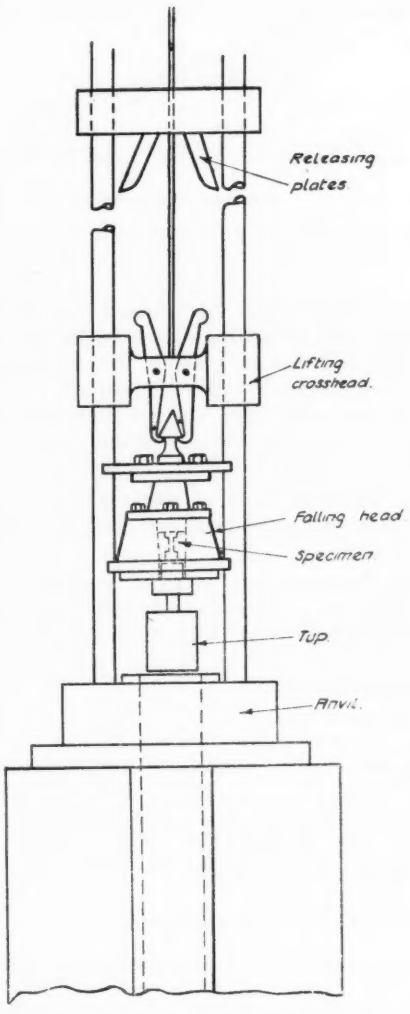


FIG. 9—Arrangement of impact machine

that when the specimen is given a blow its extension will be limited to this distance. Finally the top portion of the falling head is bolted into position.

With this apparatus the following two series of tests are available :

(1) A single impact can be split up into a number of stages by taking a number of specimens and subjecting each one to a different amount of extension (by varying the position of the stop) under the same initial conditions of impact. Each specimen may then be examined under the microscope and given various mechanical tests. In this manner the actual process of deformation during a single impact may be deduced.

(2) Repeated impact tests at approximately constant velocities of deformation can be carried out. In all previous work on repeated impact the velocity of deformation varied from a maximum at the initial stage of the impact to zero at the end, when the tup, or hammer, is brought to rest by the specimen. With the present apparatus, however, the energy available in the tup is much larger than that required to deform the specimen. This means that the tup suffers only a very small change of velocity in deforming the specimen. The rest of the energy in the tup, however, is taken by that portion of the specimen between the stop and the tup. Therefore this portion is made much longer for repeated impact specimens so that it can absorb the extra energy without fracturing. That there should be constant velocity of deformation during impact is essential, otherwise too many variables are present to enable one

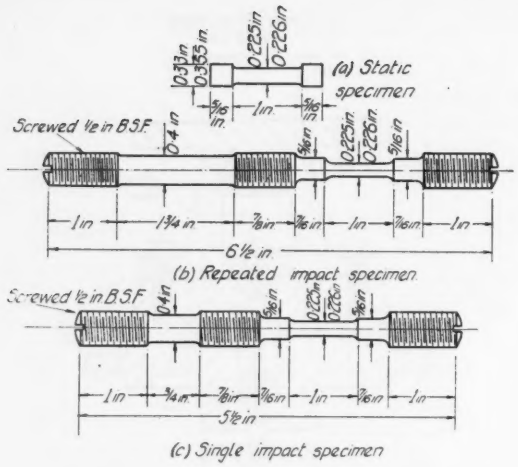


FIG. 11—Details of specimens

to draw any conclusions regarding the deformation observed in subsequent tests.

Specimens

The various specimens used are shown in Fig. 11. Figs. 11 (*a*) shows the static specimen and Figs. 11 (*b*) and 11 (*c*) the impact specimens for repeated and single impact respectively. Any impact specimen, after initial impact can be made to fit the tensile-testing machine simply by cutting-off the screwed ends.

Table II gives the chemical analysis and the mechanical properties of the metals tested.

Method of Testing

On the impact specimens two small flats were filed diametrically opposite along the whole of the gauge length. One of these flats was given a high metallurgical polish and the specimen was then placed in the impact machine, if the spread of deformation was to be studied by the microscope ; but if Vickers hardness numbers along the specimen were to be taken to denote deformation,

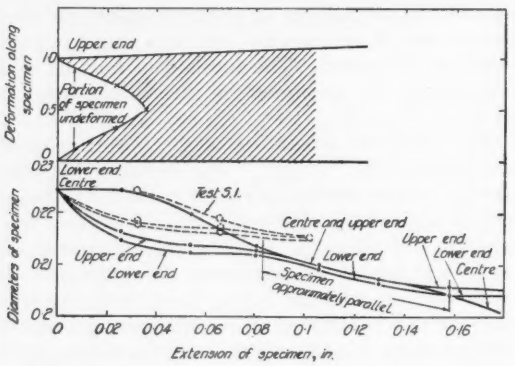


FIG. 12—Extension per blow (half turn of stop). Test *D* 13 on mild steel. Velocity of impact 133 in./sec.

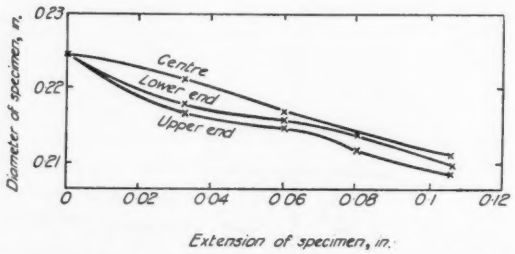


FIG. 13—Extension per blow (half turn of stop). Test *D*14 on mild steel. Velocity of impact 266 in./sec.

then the gauge length was divided into 30 equal divisions, and a hardness test taken in every third division before placing the specimen in the impact machine. After the impact the specimen was removed from the machine and tested as follows ; item (1) was carried out in each case, followed by one of items (2), (3), or (4) :

(1) The diameters at various points along the specimen were measured, using a pointed micrometer.

(2) The unetched polished surface of the specimen was viewed under the microscope after every impact. This gave the slip in the

TABLE II—*Details of Materials Tested*

(Dia. of specimens 0·224 in., gauge length 1 in.)

Condition	Yield Stress, tons/sq. in.	Max. Stress, tons/sq. in.	Fracture, tons/sq. in.	Elongation, %	Reduction in Area, %
(1) *Mild Steel* (*Bright-Drawn Bar*) : 0·22% *C*, 0·49% *Mn*, 0·025% *S*, 0·031% *P*					
As received	31·8	32·8	23·8	15	58
Normalized	16·5	26·2	18·65	39	60
(2) *Lowmoor Iron* (*Hot-Rolled Bar*): 0·085% *C*, 0·14% *Si*, 0·136% *S*, 0·136% *P*, 0·01% *Mn*					
As received	13·8 (upper yield) 12·6 (lower yield)	20	15·8	40	66·5

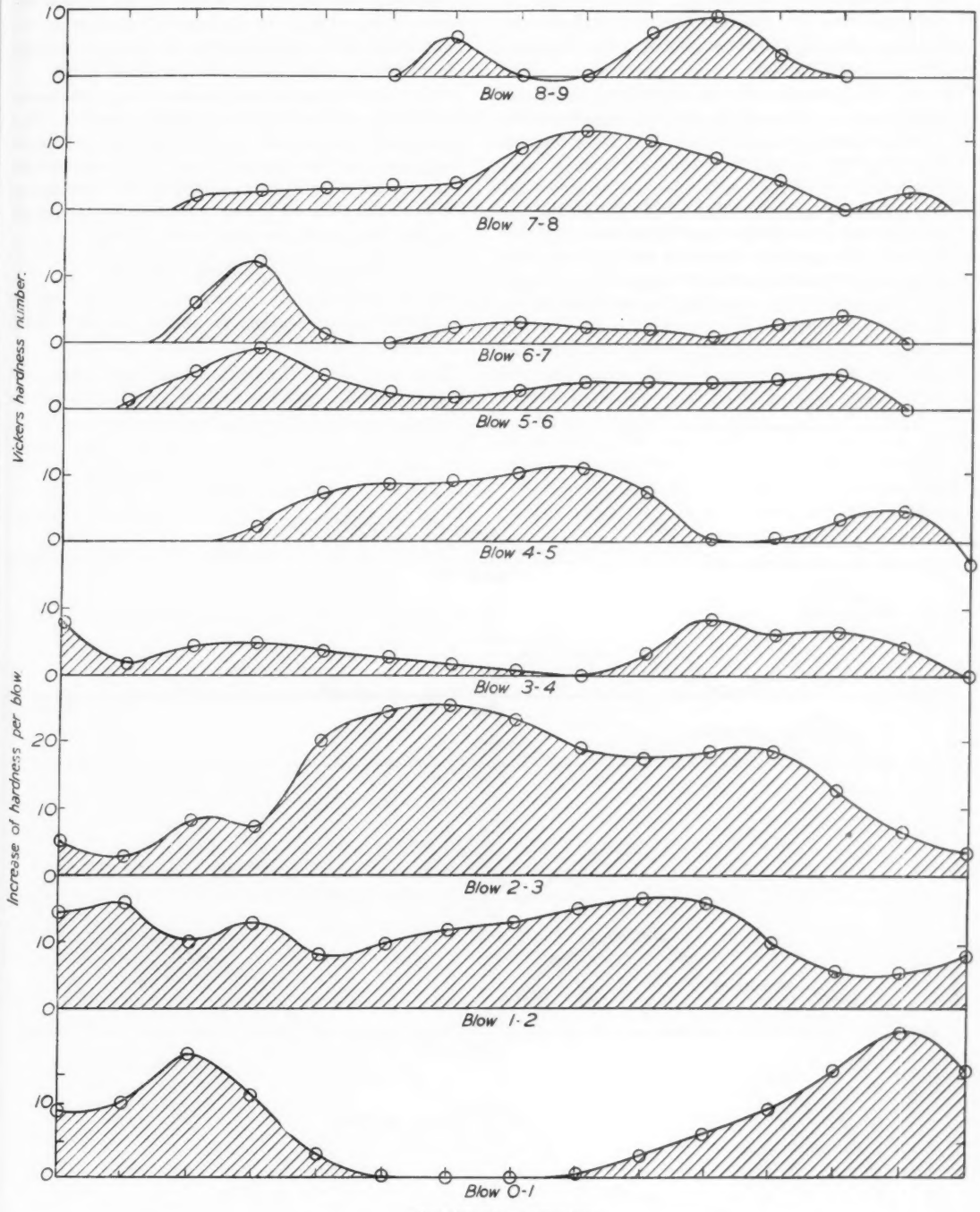


FIG. 14—Extension per blow (half turn of stop). Test *D*13 on mild steel, half maximum velocity

surface crystals and the manner in which the deformation was spreading. In the case of repeated impact the surface was repolished before replacing the specimen into the machine for the next impact.

(3) The spread and intensity of the deformation produced by each impact was measured by Vickers hardness tests before and after impact in every third division (the specimen was divided as explained above). To ensure that the polishing operation did not work-harden the surface and over-shadow any change in hardness due to deformation, hardness tests were taken at a point remote from the gauge length with various loads. This made the diamond penetrate to various depths. If there had been a work-hardened film on the specimen then the hardness number obtained with a small indent should have been greater than that obtained with a large indent. If the hardness number remained constant, independent of the depth of indent, then it was taken to indicate that if a work-hardened film were present it was too minute to effect the validity of the hardness tests.

(4) The hardness was taken along the centre and edge of the specimen after impact. The specimen was cut so that the metal had equal support whether the hardness tests were taken at the edge or at the centre of the specimen. The surfaces were given a metallurgical polish before the hardness tests were taken.

Tests on Normalized Mild Steel

Effect of Velocity of Impact upon the Spread of Deformation During Repeated Impact—Tests D13 *and* D14

For test *D*13 the extension per blow was approximately 0·035 (one-half turn of stop) and

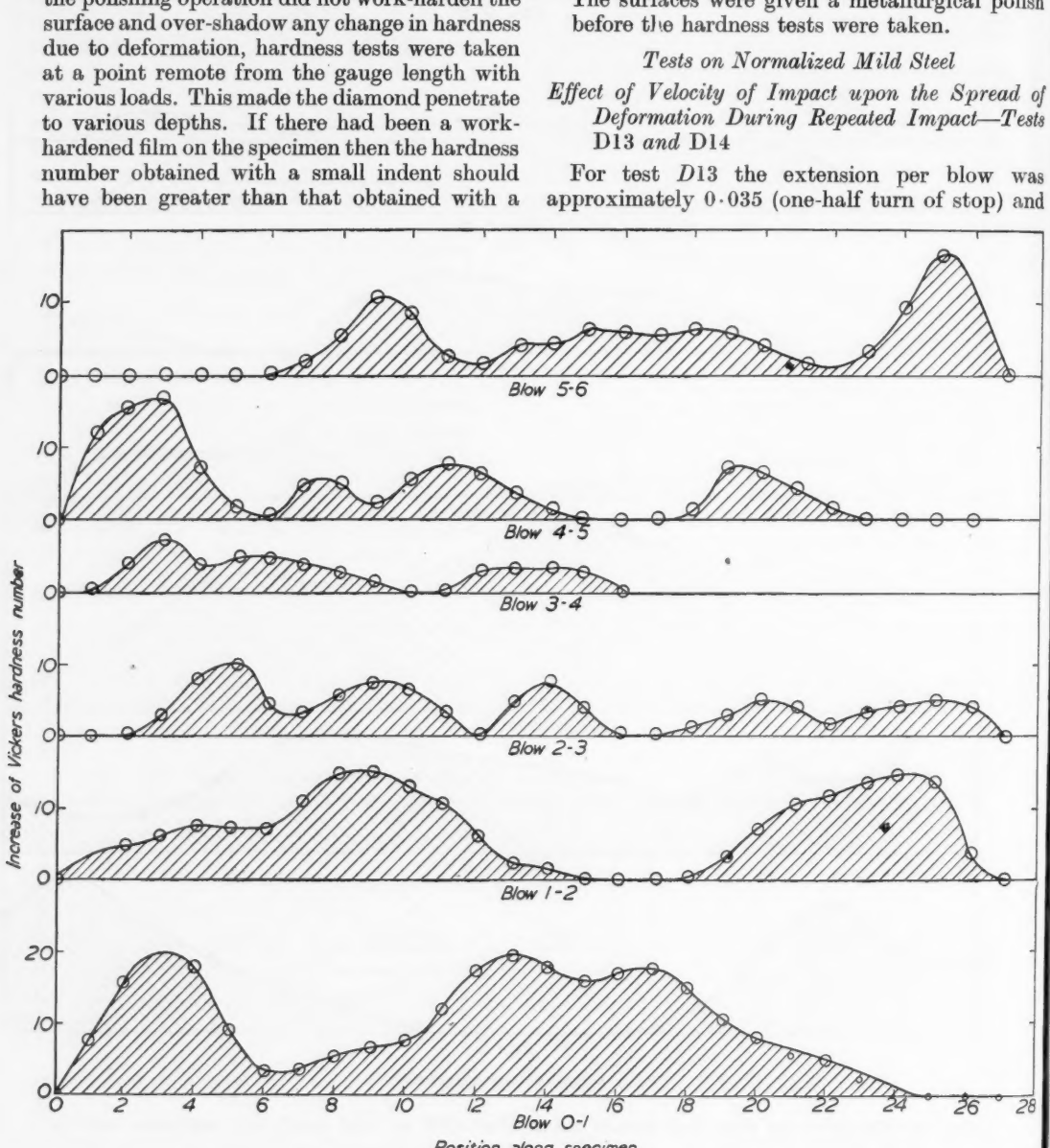


FIG. 15—Increase in hardness along specimen per blow. Extension per blow, half turn of stop. Maximum velocity

the velocity of impact 133 in./sec. Test $D14$ was the same as $D13$ except that the velocity of impact was doubled (266 in./sec.). The results of these tests are plotted in Figs. 12 and 13 respectively. The results for the hardness tests carried out on these specimens are shown in Figs. 14 and 15.

The first point to be observed from the results of tests $D13$ and $D14$ is that when the velocity of impact is a maximum (266 in./sec.) the maximum deformation occurs at the falling-head end of the specimen (equivalent to the hammer end), while when the velocity of impact is only 133 in./sec. the maximum deformation occurs at the tup end of the specimen (equivalent to the fixed end).

Consider test $D14$. In this case V/V_y (Table I) equals 0·587, which gives point P_{14} on Fig. 16. A horizontal line through P_{14} cuts the velocity curve (e) at Q_{14}, and a vertical line through Q_{14} cuts the deformation curves (a) and (b) and the horizontal axis at U_{14}, T_{14}, and V_{14}, respectively. It will be seen at a glance that in this case $U_{14}V_{14} > T_{14}V_{14}$ and therefore the greater deformation will occur at the head end. (Curve (a) applies to the head end and curve (b) to the tup end.)

Next consider test $D13$. For this test V/V_y equals 0·312 (Table I) and locates point P_{13} on Fig. 16. A horizontal line through P_{13} cuts the velocity curve at Q_{13} and a vertical line through this point cuts the curves (a) and (b) and the horizontal axis at points U_{13}, T_{13}, and V_{13}, respectively. It will be noted that in this case $T_{13}V_{13} > U_{13}V_{13}$. Therefore the greater deformation will take place at the tup end of the specimen.

Another point of interest revealed by tests $D13$ and $D14$ is that with repeated impact on mild steel two necks are first formed on the specimen. Deformation spreads along the specimen until at an extension of 0·082 in. the specimen is again parallel. The specimen remains parallel until an extension of 0·158 in. is reached (Fig. 12). Beyond this extension a new neck is formed, approximately at the centre of the specimen, which eventually fractures. Thus the final specimen has no indication of the initial double necking. It will also be seen from the hardness tests, Figs. 14 and 15, that the two necks are more definite at low than at high velocities, and, further, that,

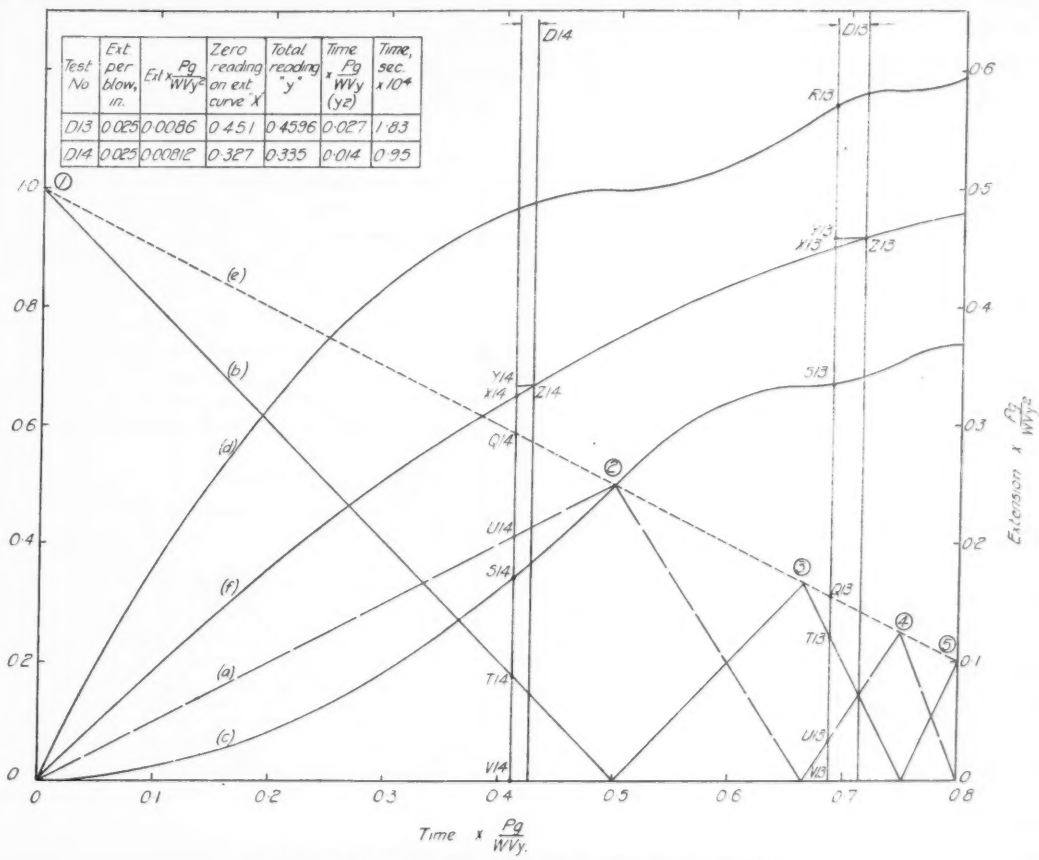


FIG. 16—Conditions of impact for tests $D13$ and $D14$

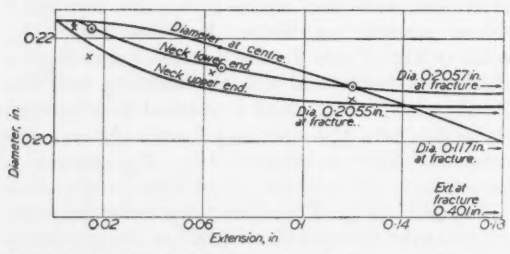


FIG. 17—Analysis of deformation during a single impact on mild steel. Tests *D*29 to *D*32. Velocity of impact 266 in./sec.

apart from the initial spread of deformation, the deformation per blow is more isolated with higher velocities of impact, but the deformation at each blow takes place at different points along the specimen, thus keeping the specimen more parallel (Fig. 13).

Effect of Radius at End of Specimen—Test S1

Since, according to the theory, the necking at the ends of the specimen was due to the reflection of the stress wave, it is logical to expect that the radius of the fillets at the end of the gauge length would have some effect upon the spread of deformation.

In order to check this, a special specimen was made (*S*1) with $\frac{1}{2}$-in. radii at the ends and a gauge length of 1·4 in. The conditions of the test were the same as for *D*13 and the results are plotted on Fig. 12 from which it will be seen that the deformation is less concentrated at the ends in test *S*1 than in *D*13; thus it may be concluded that the larger the radius at the ends of the gauge length, the less pronounced will be the initial necks. Specimens with 2-in. radii at the ends were broken by repeated impact, and there was still evidence of "double necking." The percentage reduction in areas for the pronounced double necking were as follows: At fracture, 66%; at second neck, 34%; and at parallel portion, 27%.

Analysis of Single Impact on Mild Steel—Tests D28, D29, D30, D31, *and* D32 (*Velocity* = 266 *in./sec.*)

By taking a number of specimens and setting the stop for a different extension for each specimen, the process of the deformation during a single

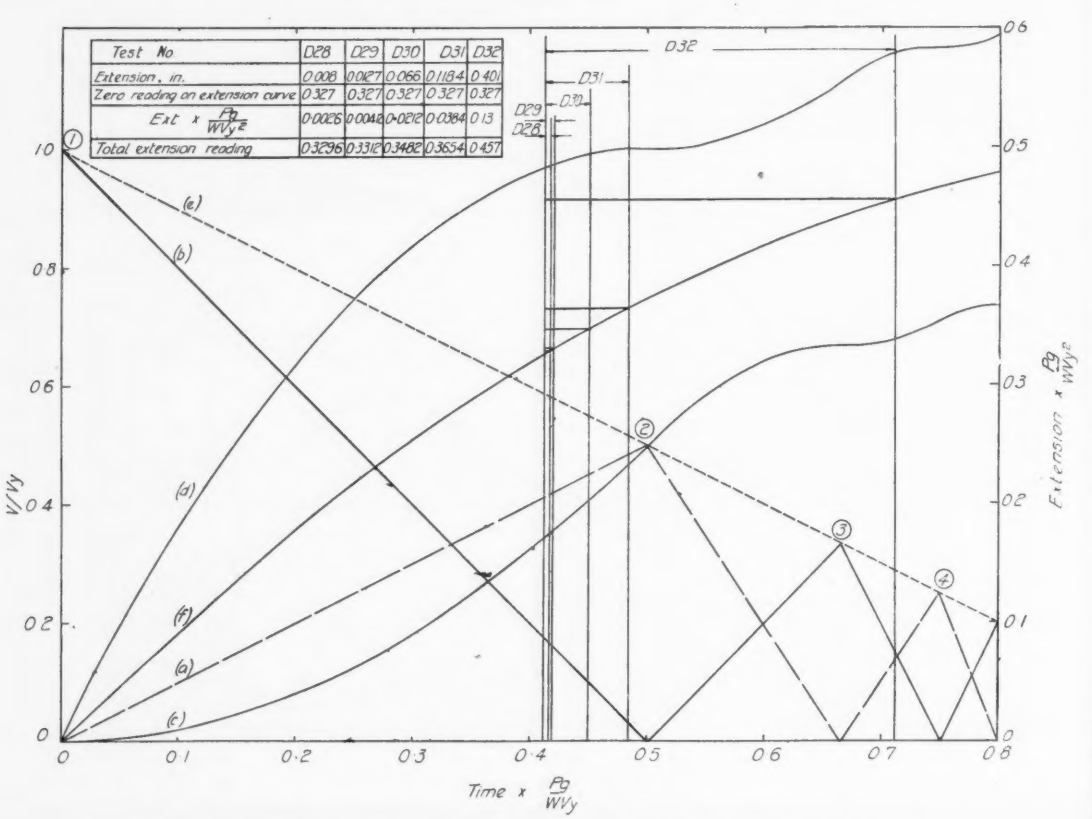

Test No. | D28 | D29 | D30 | D31 | D32
Extension, in. | 0·008 | 0·0127 | 0·066 | 0·1184 | 0·401
Zero reading on extension curve | 0·327 | 0·327 | 0·327 | 0·327 | 0·327
Ext × Pg/WVy² | 0·0026 | 0·0048 | 0·0212 | 0·0384 | 0·13
Total extension reading | 0·3296 | 0·3312 | 0·3482 | 0·3654 | 0·457
D32
D31
D30
D29
D28
(a)
(b)
(c)
(d)
(e)
(f)
V/Vy
Time × Pg/WVy
Extension × Pg/WVy²

FIG. 18—Conditions of impact for tests *D*28 to *D*32, mild steel, single impact

impact may be analysed. The results of such tests are shown in Fig. 17. Figure 18 shows the mechanism of the deformation of specimens *D*28, *D*29, *D*30, *D*31, and *D*32. These diagrams show nothing of particular interest except that the deformation takes place predominantly at the head end of the specimen. In the case of specimen *D*32 the reflections change from (1) and (2) to (3) and (4) during the final part of the impact.

It will be noted, however, from Fig. 17 that, although double necking occurs in mild steel during a single impact, the necks disappear and the specimen becomes parallel again at an extension of 0·12 in. Beyond this extension a fresh neck occurs which eventually fractures, the remaining deformation being localized at this neck. Therefore the final fractured specimen shows no indication of the double necking which has taken place during the early stages of the impact.

Tests on Lowmoor Iron

Owing to the clearness of the slip bands obtained with Lowmoor iron, this metal was used to investigate the difference in slip bands with impact and static loading. All specimens had a gauge length of 1 in. and were 0·225 in. in dia.

Preliminary Tests—Comparison of Deformation During Impact and Static Tests. Impact Tests D8, D9, *and* D10

In tests *D*8, *D*9, and *D*10, the stop was set for all specimens so that the extension was approximately 0·034 in., but different velocities of impact were used for each specimen. The results of these tests are given in Table III.

Static Test D12

A specimen with a polished flat surface was placed in the static testing machine (the dimensions of the specimen were the same as for the impact specimen) and the load was applied until it had extended 0·018 in. (end of plastic yield); the load was then removed and the surface examined for slip bands. The specimen was replaced in the machine and loaded again until its total extension was 0·034 in.; it was then removed and the microstructure of the surface again examined. This process was repeated four times, the specimen being loaded until the diameter had been reduced to the following respective values: 0·217, 0·215, 0·21, and 0·203 in., these being equal, as far as possible, to the minimum diameters obtained in the impact tests *D*8, *D*9, and *D*10.

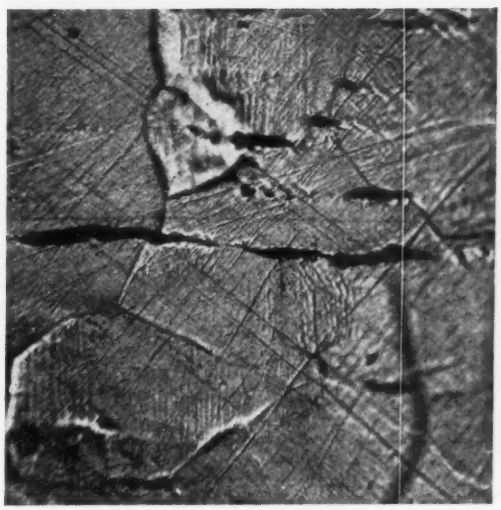


FIG. 19—Lowmoor-iron impact test (test *D* 8). Extension 0·034 in., minimum dia. 0·203 in.

Deformation at Surface of Specimen

Figure 19 shows the deformation at the point of maximum deformation for test *D*8 (*i.e.*, at the point where the diameter had been reduced from 0·223 to 0·203 in.); Fig. 20 shows the slip bands in

TABLE III—*Results of Tests* D8, D9, *and* D10, *on Lowmoor Iron*

Every specimen was subsequently examined for slip bands at the point of maximum deformation

Test	Impact Velocity, in./sec.	Extension, in.	Initial Dia., in.	After Impact		Length of Disturbance, in.
				Max. Dia., in.	Dia. at Necks, in.	
*D*8	264	0·034	0·225	0·225	0·203* 0·2165†	0·346* 0·2875†
*D*9	132	0·037	0·226	0·226	0·215* 0·215†	0·434* 0·2955†
*D*10	66	0·031	0·226	0·226	0·219* 0·21†	0·24* 0·348†

* Upper portion of gauge length † Lower portion of gauge length

FIG. 20—Lowmoor-iron static test (test *D*12). Extension 0·157 in., minimum dia. 0·203 in.

FIG. 21—Lowmoor-iron static test (test *D*12). Extension 0·018 in. (end of plastic yield)

the surface of the static specimen *D*12 when the minimum diameter was 0·203 in., *i.e.*, the same as impact test *D*8 ; Fig. 21 shows the slip bands in the surface at the end of plastic yield in a static test. There are signs of slip between and in the crystals. The slip between the crystals may be due to the accommodation of the crystals after their change of shape due to internal slip, or on the other hand, plastic yield may be partly produced by slip between the crystals. By comparing Figs. 19 and 20 it will be noted that, for the same degree of deformation, many more slip bands are produced in the static test than in the impact test.

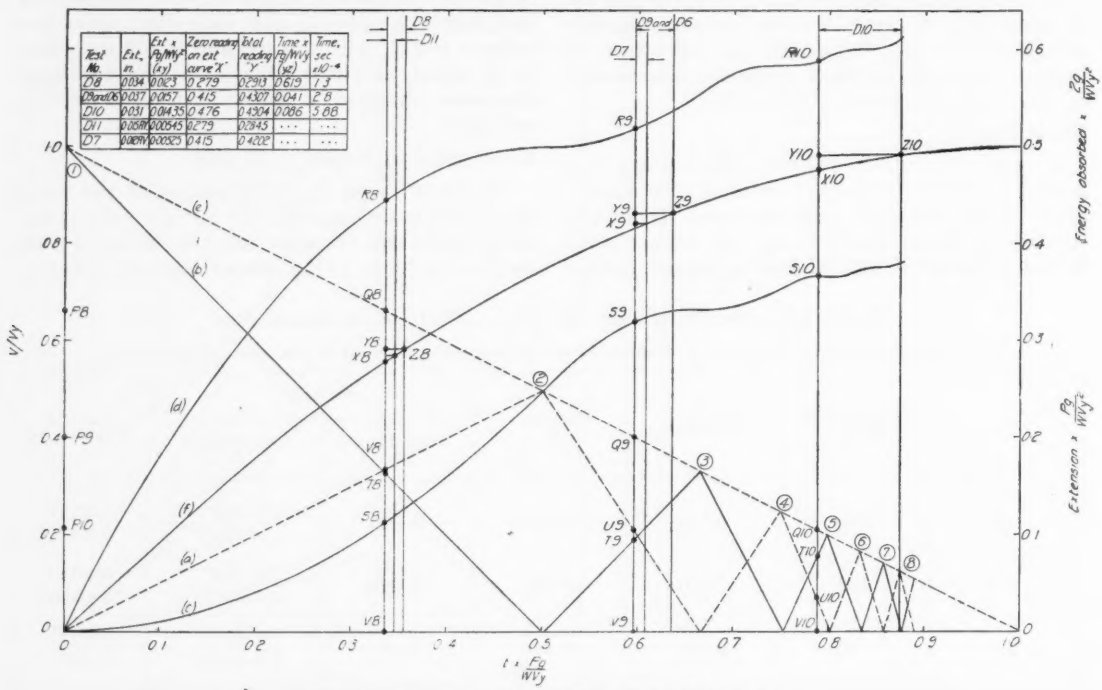

Test No. | Ext. in. | Ext. x Fg/WVy² (xy) | Zero reading on ext. curve 'X' | Total reading 'Y' | Time x Fg/WVy (yz) | Time, sec ×10⁻⁴
D8 | 0·034 | 0·0123 | 0·279 | 0·2913 | 0·619 | 1·3
D6 and D6 | 0·037 | 0·057 | 0·415 | 0·4307 | 0·041 | 2·8
D10 | 0·031 | 0·04435 | 0·476 | 0·4904 | 0·086 | 5·88
D11 | 0·058 | 0·00545 | 0·279 | 0·2945 | ... | ...
D7 | 0·087 | 0·00525 | 0·415 | 0·4202 | ... | ...
V/Vy
t = Fg/WVy
Energy absorbed = Fg/WVy²
Extension = Fg/WVy²

FIG. 22—Impact conditions for tests *D*6 to *D*11

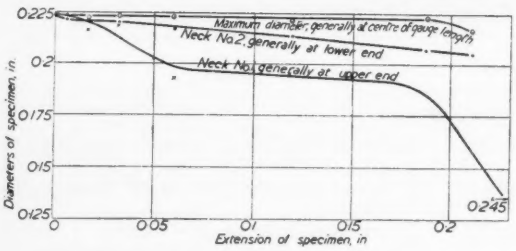


FIG. 23—Analysis of deformation during single impact

Figure 22 shows the location of tests *D*8, *D*9, and *D*10 on the theoretical diagram. It will be seen that for *D*8 maximum deformation is predicted at the head end. The rates of deformation are approximately equal at the commencement of impact, but, as deformation proceeds, the major portion of the deformation occurs at the head end. This is in agreement with the test results. For test *D*9 maximum deformation is predicted at the tup end. This is contrary to experimental results, which showed that the amount of deformation at each end was approximately equal. This may be due to the uncertainty concerning the values

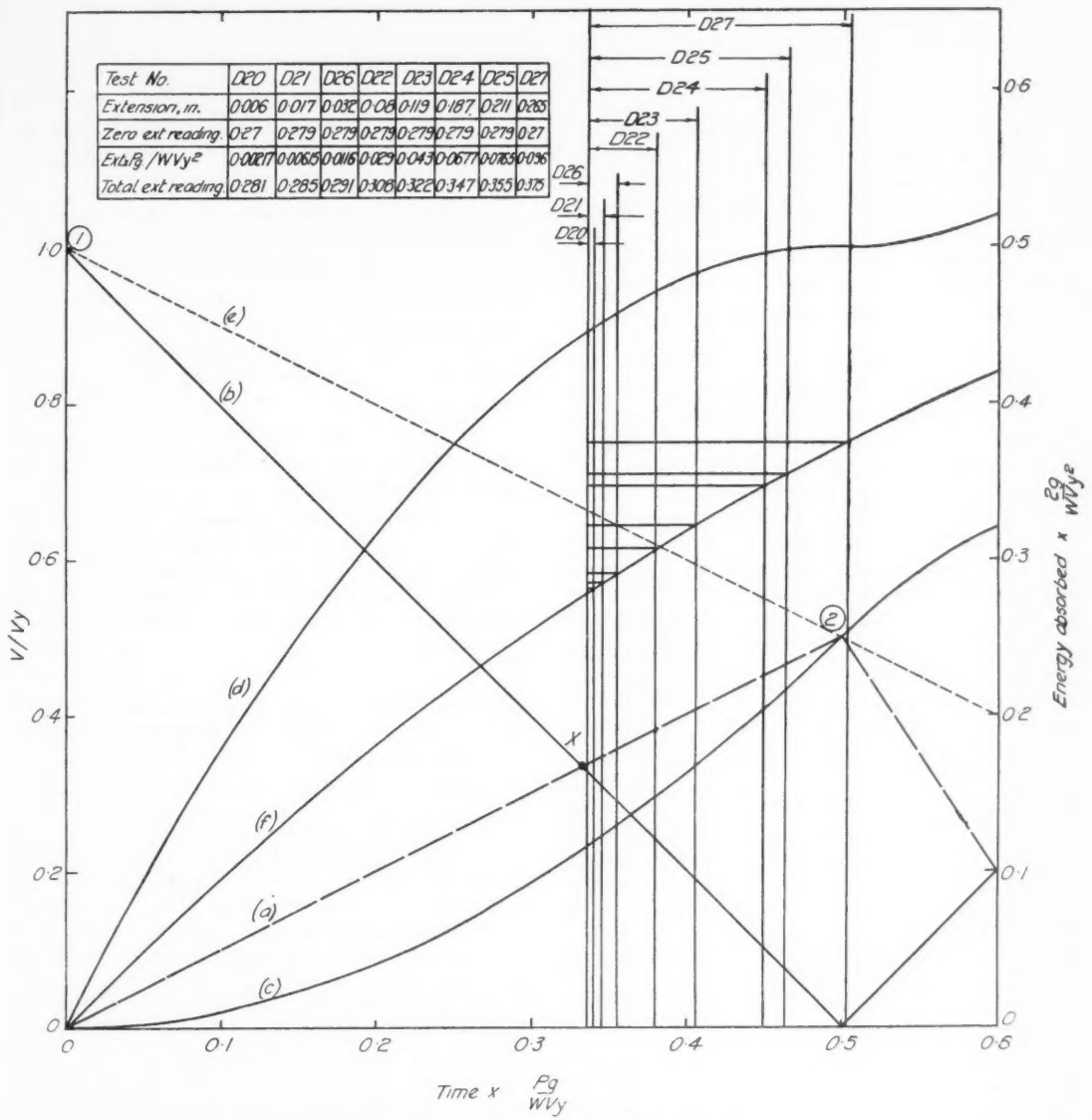


FIG. 24—Conditions of impact for tests *D*20 to *D*27

TABLE IV—*Maximum Velocity of Impact*

Test No.	Initial Dia., in.	Extension, in.	Min. Dia., Upper End, in.	Min. Dia., Lower End, in.	Max. Dia., in.	Remarks
*D*20	0·222	0·006	0·222	0·22	0·222	Deformation along $\frac{2}{3}$ of length from lower end
*D*21	0·224	0·017	0·217	0·221	0·224	Deformation along $\frac{1}{3}$ of length from each end
*D*26	0·223	0·032	0·219	0·220	0·222	Deformation over whole specimen
*D*22	0·224	0·06	0·194	0·218	0·224	Deformation along $\frac{1}{3}$ of length from each end
*D*23	0·223	0·119	0·21* 0·195	0·214	0·2225	Deformation over whole length. Slight neck at end and middle
*D*24	0·223	0·187	0·196*	0·207	0·210	Deformation over whole length
*D*25	0·223	0·211	0·138	0·206	0·2225	Specimen broken at upper end
*D*27	0·224	0·265	0·136	0·207	0·217	Specimen broken at upper end

* Diameter at centre of gauge length

taken for σ_y with Lowmoor iron. A value of 0·41 for V/V_y is required to predict equal deformation at each end, which corresponds to a value of σ_y equal to 21·0 tons/sq. in., that is, 0·6 tons/sq. in. less than that obtained from Smith and Warnock's results.

The diagram predicts for test *D*10 slightly greater deformation at the tup end, which is consistent with experimental evidence.

Analysis of Single Impact Tests—D20 *to* D27

Tests *D*20 to *D*27 were carried out in the same manner as tests *D*29 to *D*32. The results are given in Table IV and are also shown graphically in Figs. 23 and 24. The points in Fig. 23 are fairly scattered, but since a different specimen had to be used for each test, this must be expected.

Figure 24 shows the process of deformation which takes place in each specimen. An interesting point is that for specimen *D*20, which was extended only 0·006 in.; the maximum deformation was at the lower end, but for all other tests the maximum deformation occurred at the head end of the specimen. The reason for this can be seen from Fig. 24. If the impact commences slightly to the left of the point for equal deformation at each end (*i.e.*, point *X* on Fig. 24 where curves (*a*) and (*b*) cross) then for small extensions, the maximum deformation is at the tup end, but for larger extensions the period of deformation extends further to the right of *X*, which causes the maximum deformation to be produced at the head end and not at the tup end, as it was during the initial stages of the impact. The fact that the origins of the impacts do not lie to the left of *X* is another indication that the value of σ_y obtained from Fig. 8 is slightly high.

Hardness tests were also taken along each specimen before and after impact so that the variation of work-hardening along each specimen was obtained.

These tests revealed local hardening due to local deformation, in a manner which would be anticipated from Table IV. The other points of interest revealed by Fig. 23 are :

(*a*) The deformation has not spread over the whole length of the specimen until an extension of about 0·12 in. has been reached.

(*b*) From an extension of 0·07 to 0·18 in. the deformation continues at approximately a uniform rate over the whole length of the specimen (*i.e.*, the curves in Fig. 23 all have the same slope).

(*c*) Beyond an extension of 0·18 in. the larger of the two necks deforms more rapidly until fracture occurs.

(*d*) Although both specimens *D*25 and *D*27 fractured at the upper end, specimens *D*23 and *D*24 (Table IV) would have fractured at the centre if deformation had been allowed to continue. Thus the point of fracture seems uncertain.

Effect of Extension per Blow and of Velocity of Impact on Spread of Deformation during Repeated Impact

Tests D6, D7, *and* D11

From the results of tests *D*8, *D*9, and *D*10, the spread of deformation depends upon the velocity of impact, as well as the extension. Test *D*6 examined the formation of necks and the spread of deformation for a fall of 23 in. (velocity of impact 133 in./sec.) and an extension of approximately 0·035 in. per blow (one-half turn of stop).

The results for this test are given in Fig. 25. Test *D*7 is the same as *D*6 except that the extension per blow is approximately 0·01 in. (one-eighth turn of stop). These results are plotted in Fig. 26. Specimen *D*11 was treated similarly to *D*7, except that the velocity of impact was

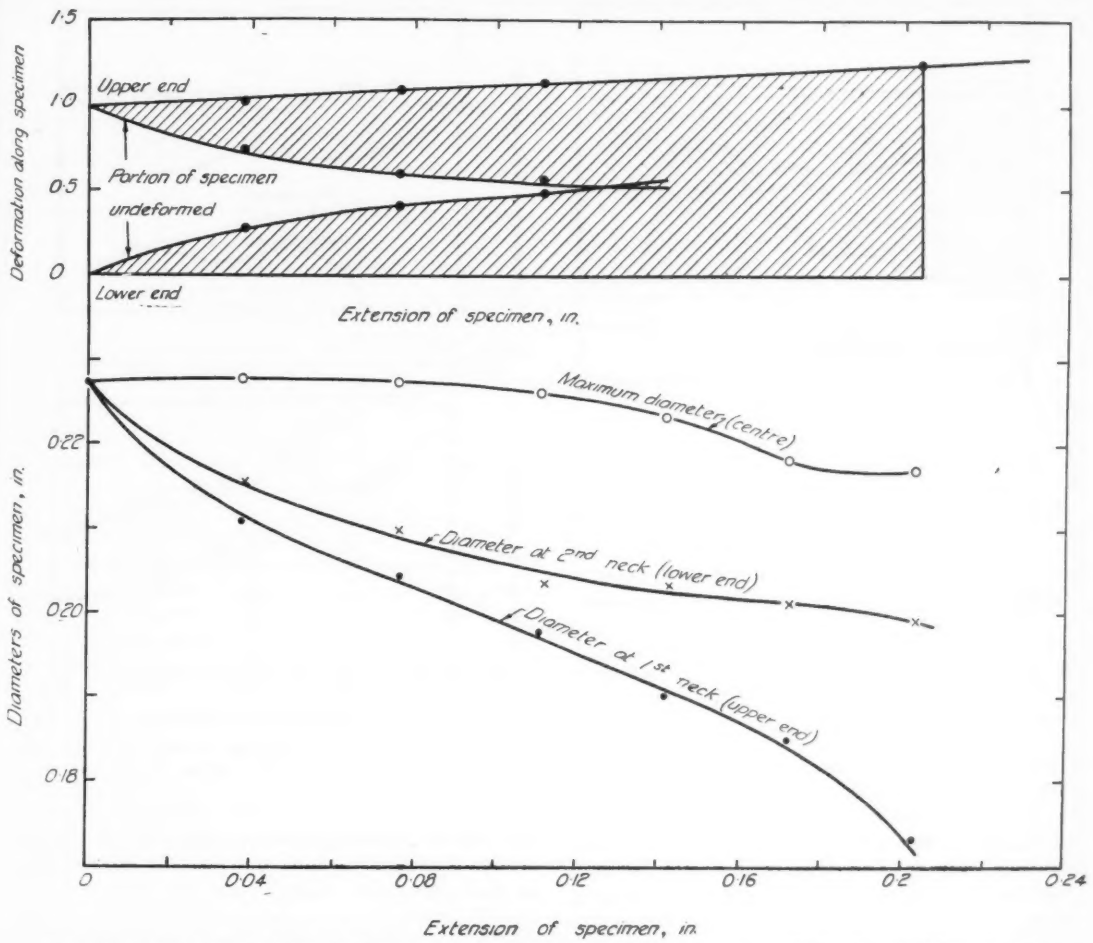


FIG. 25—Extension per blow (half turn of stop). Specimen *D*6, Lowmoor iron, 23-in. drop, half maximum velocity

doubled (266 in./sec.). The results of this test are plotted in Fig. 27.

The processes of deformation for these tests are shown in Fig. 22. Referring to Figs. 26 and 27, it will be noted that the deformations at each end are approximately equal for test *D*7, but differ rather for test *D*11. It will also be noted that deformation occurs more rapidly at the centre of the gauge length for low velocities than for high velocities.

Comparison of Single Impacts with Mild Steel and Lowmoor Iron

The first point of interest is that for mild steel deformation has spread over the whole specimen after only a small extension, Fig. 17, while for Lowmoor iron deformation has not spread over the whole specimen until an extension of 0·187 in., (denoted by commencement of reduction in diameter at the centre of the gauge length), the specimen breaking at an extension of 0·265 in., Fig. 23. Secondly, it will be noted that the Lowmoor iron specimens never become parallel after the initial necking, but that in most cases (there have been a few exceptions) the specimen fractures at one of the original necks, while the mild-steel specimens generally become parallel again after the first double necking, and then a third neck is produced at the centre of the specimen, where fracture eventually occurs. Also, for mild steel, the rate of spread of deformation decreases as the velocity of impact is decreased, which is the reverse of that in the case of Lowmoor iron. These phenomena are probably due to the inaccuracy of assumption (1).

Assumption (1) cannot be strictly true for either metal, otherwise the deformation would never spread from the ends at all, but it is very nearly true for Lowmoor iron. In the case of single-impact tests on Lowmoor iron where the height

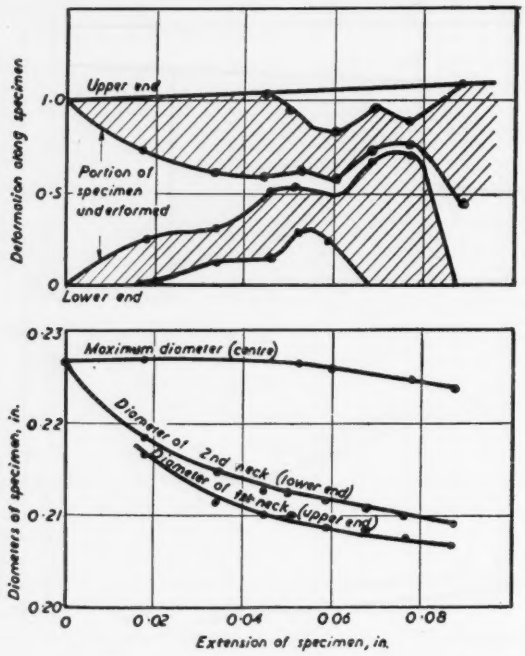


FIG. 26—Extension per blow (one-eighth turn of stop). Test *D*7, Lowmoor iron, 23-in. drop, half maximum velocity

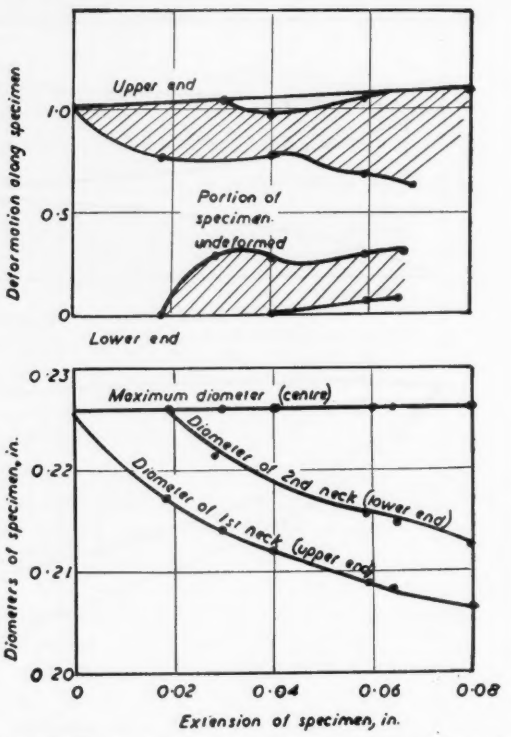


FIG. 27—Extension per blow (one-eighth turn of stop). Test *D*11, Lowmoor iron, 92-in. drop, maximum velocity

of fall was about 110 in., the specimen had two severe necks, one at each end, but the centre of the specimen had not been stressed beyond its elastic limit. For mild steel, however, the error is rather greater, and with this metal there is evidence of plastic waves, as well as the elastic waves, being propagated from each end of the specimen. It is the interaction of these plastic waves which first brings the specimen parallel and then finally produces fracture at the centre of the gauge length.

When two elastic waves which are travelling in opposite directions along the same bar, meet, and overlap, the total effect due to the overlapping is the algebraic sum of the two. Each wave is unaffected by the other either in velocity of propagation or magnitude. This is because the velocity of propagation is independent of strain. With plastic waves, however, the velocity of propagation is a function of strain, and when two such waves meet, and overlap, the strain in that portion is increased which reduces the velocity of propagation of each wave, and, in order to facilitate the velocity of impact, the magnitude of the strain at the junction of the two waves must be increased still further, which in turn reduces the velocity of propagation even more. This obviously is a state of instability. Thus when two plastic wave fronts meet, the strain at the junction of the two fronts is continually increasing until eventually the strain is such that $\delta\sigma/\delta\xi$ is zero and fracture is produced at that point.

Effect of Gauge Length on Impact-Percentage-Elongation/Static-Percentage-Elongation and Impact-Percentage-Reduction-in-Area/Static-Percentage-Reduction-in-Area Ratios

From the double necking which occurs during the early stages of impact, it appears that during impact more deformation takes place outside the final neck than is the case in a static test. This would suggest that the ratio :

$$\frac{\text{Impact percentage elongation}}{\text{Static percentage elongation}}$$

would be a function of the gauge lengths used as well as of the material. In order to investigate this, several specimens of various gauge lengths were made both for the impact and for the static testing machine. The steel used was a piece of hot-rolled mild steel and all specimens were cut from one bar. The results of the impact and static tests are plotted in Fig. 28.

The average percentage reductions in area at the point of fracture are 59·96 for impact and 60·8 for static tests, and outside the neck 24·85 and 23·23 respectively. Thus we see that for

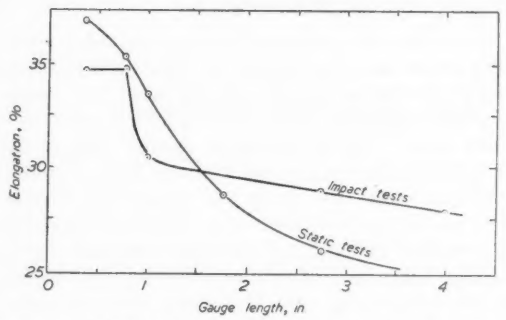


FIG. 28—Effect of gauge length on percentage elongation during impact and static tests

impact the percentage reduction in area outside the neck is greater than that for the static test, while the percentage reduction in area at the neck is less in impact than in static tests.

Figure 28 shows clearly that by choosing a suitable gauge length practically any ratio of impact percentage elongation to static percentage elongation could be obtained. This particular steel would give a ratio of unity if a gauge length of about $1\frac{1}{2}$ in. was used.

Hardness Tests on Partially Shocked Mild-Steel Specimens

From previous work it was noted that in both static and impact tests the centre crystals are deformed more than the surface crystals, or at least, show a greater increase of hardness.

The process of deformation during impact is very complex. At first a local neck occurs, which makes the centre crystals of the specimen become more work-hardened than those at the surface, but as soon as the neck is formed there is a stress concentration at the surface, due to the local reduction in cross-sectional area, which will tend to produce more deformation at the surface than at the centre. Thus in a broken impact specimen a combination of these two effects is being measured. If, then, it is required to isolate the effect of impact, the deformation must be stopped immediately the local neck has formed. In order to investigate this, hardness tests were taken along the edge and centre, as in previous tests, for mild-steel specimens *D*29, *D*30, and *D*31, corresponding to extensions of 0·0127, 0·066, and 0·1184 in., respectively. The results of these tests are shown in Figs. 29, 30, and 31, respectively.

Figure 32 shows the variation between the hardness difference at the centre and at the edge with reduction in diameter for impact specimens. It will be seen that it rises to a maximum of 19·8 and then falls sharply again. The difference in the static test remains constant at about 14 over a wide change of diameters. The diameter taken in

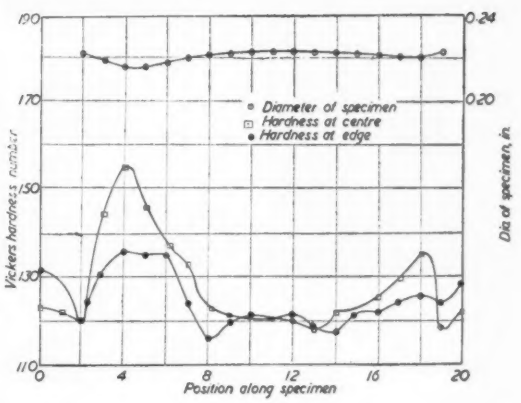


FIG. 29—Test *D*29, mild steel, maximum velocity, extension 0·0127 in.

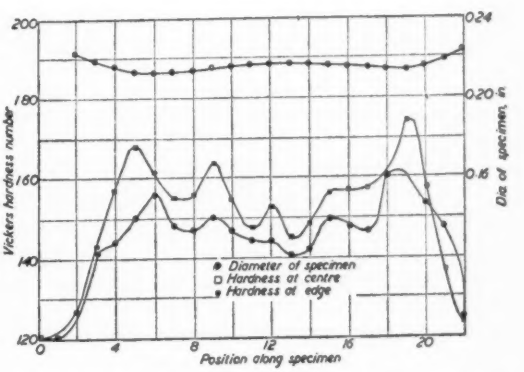


FIG. 30—Test *D*30, mild steel, maximum velocity, extension 0·066 in.

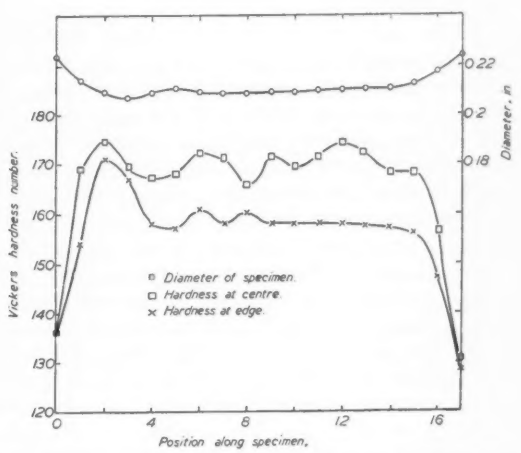


FIG. 31—Impact test *D*31 on mild steel. Extension 0·1184 in.

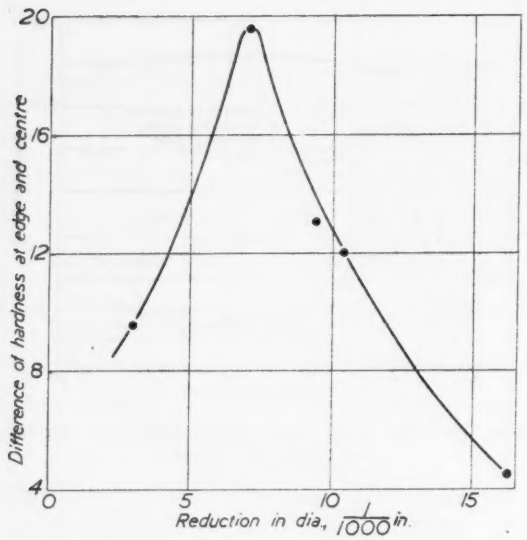


FIG. 32—Excess of hardness at centre of specimen during impact for various reductions in diameter

each case is the minimum diameter of the specimen.

The axial extension must be constant across the whole cross-section. If it is assumed that the increase of hardness for a given axial extension is less when slip takes place between the crystals than when it takes place in the crystals, then the difference in hardness at the centre and edge of a strained specimen is co-related with the formation of slip bands.

From tests carried out with brass, which has no plastic yield, the difference of hardness between the centre and edge was found to be very small and little affected by the method of straining the specimen.

There seems to be some indication that with metals which have "plastic yield" the mode of deformation varies across the cross-section. At the surface a larger portion of the axial extension arises from slip between the crystals than is the case at the centre. There is, however, not sufficient metallurgical evidence to draw any definite conclusions.

CONCLUSIONS

Experimental evidence has been provided which shows that the propagation of deformation during impact, whether single or repeated, is fundamentally different from that which occurs in a static test. The propagation during impact can be explained in a general manner from dynamic considerations. To apply the dynamic principles with precision, however, more accurate knowledge is required concerning the yield process during impact, the effect of rate of straining on the rate of age-hardening, and the shape of the stress–strain diagrams during impact.

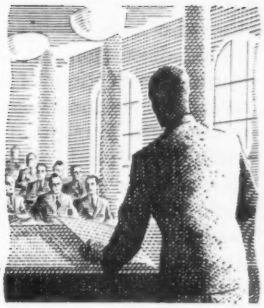
ACKNOWLEDGMENTS

Most of the experimental work was carried out at the College of Technology, Belfast, and the author wishes to express his thanks to Professor F. V. Warnock, Ph.D., for the use of his laboratories. The author's thanks are also due to the University of London for a grant towards the expenditure of this research, and finally to his assistant, Mr. D. Keily, B.Sc.

REFERENCES

1. H. C. MANN : *Proceedings of the American Society for Testing Materials*, 1935, vol. 35, part 2, p. 323.
2. A. C. VIVIAN : *Proceedings of the Institution of Mechanical Engineers*, 1947, Sept., War Emergency Issue No. 26.
3. H. C. MANN : *Proceedings of the American Society for Testing Materials*, 1936, vol. 36, part 2, p. 85.
4. J. H. SMITH and F. V. WARNOCK : *Journal of the Iron and Steel Institute*, 1927, No. II, p. 323.
5. F. V. WARNOCK and J. B. BRENNAN : *Journal of the Iron and Steel Institute*, 1943, No. II, p. 117P.
6. F. V. WARNOCK and J. A. POPE : *Proceedings of the Institution of Mechanical Engineers*, 1947, Sept., War Emergency Issue No. 26.
7. PRESCOTT : "Applied Electricity," New York, 1946 : Dover Publications.
8. TIMOSHENKO : "Theory of Elasticity." New York, 1934 : McGraw Hill.
9. A. F. C. BROWN and N. D. G. VINCENT : *Proceedings of the Institution of Mechanical Engineers*, 1941, vol. 145, p. 126.
10. D. S. CLARK and G. DATWYLER : *Proceedings of the American Society for Testing Materials*, 1938, vol. 38, part 2, p. 98.
11. G. I. TAYLOR : *Journal of the Institution of Civil Engineers*, 1946, No. 8, p. 486.
12. T. VON KÁRMÁN : "The Propagation of Plastic Deformation." International Conference of Applied Mechanics, Paris, 1946.

DISCUSSION ON PAPERS



DISCUSSION AT MEETINGS · WRITTEN CONTRIBUTIONS

Spring Meeting, 1947

Discussion on a

SYMPOSIUM ON THE HARDENABILITY OF STEEL

In conjunction with the Annual General Meeting, 1947, of the Iron and Steel Institute, arrangements were made for a "Symposium on the Hardenability of Steel," issued as Special Report No. 36, to be discussed on Wednesday, 14th May, 1947. Two sessions, at the Offices of the Institute, were held, and the President, Dr. C. H. Desch, F.R.S., presided at both.

As time would not permit of individual consideration of all the papers in the Report, four subjects, based on selected aspects, were presented by rapporteurs for discussion.

AT THE MORNING SESSION, from 10.0 A.M. to 12.45 P.M., the Introductory Address was given by Mr. D. A. OLIVER, Chairman of the Hardenability Sub-Committee of the Technical Advisory Committee of the Special and Alloy Steels Committee (Ministry of Supply). The Discussion on "Reproducibility of End-Quench Hardenability Curves," based on Conclusions (1) to (6), pp. 413–414 of the Report, was introduced by Dr. W. STEVEN, and the Discussion on "The Influence of Chemical Composition on Hardenability," based on Conclusions (7) and (8), pp. 414–415, was introduced by Mr. J. GLEN.

AT THE AFTERNOON SESSION, from 2.15 P.M. to 5.00 P.M., the Discussion on "The Prediction of Hardness and Transformation Ranges from End-Quench Hardenability Curves," based on Conclusions (9), (10), and (12), pp. 415–416, was introduced by Mr. T. F. RUSSELL, and the Discussion on "The Influence of Hardenability on Mechanical Properties," based on Conclusion (11), p. 415, was introduced by Mr. W. E. BARDGETT.

A Buffet Luncheon was held at the Institute between the two sessions.

PROCEEDINGS OF THE MORNING SESSION: 10 A.M. TO 12.45 P.M.

INTRODUCTORY ADDRESS

Mr. **D. A. Oliver** (Chairman of the Hardenability Sub-Committee of the Technical Advisory Committee of the Special and Alloy Steels Committee, Ministry of Supply), in the course of a general introduction of the Report, said: When a small mission of five British metallurgists, led by Mr. C. R. Wheeler, C.B.E., and accompanied by Mr. D. L. Burn as Secretary, visited the United States and Canada during the Spring of 1943, the main objectives were ferro-alloy economy and a mutual exchange of experiences. Our American friends were extensively using the end-quench test, introduced by Jominy just before the war, to decide how far they could go in the attenuation of alloys in low-alloy constructional steels. Very little experience of this test was available in Great Britain, but on the return of the mission the matter seemed to the Technical Advisory Committee to be of sufficient importance to justify its immediate submission to a special sub-committee to explore its potentialities, and with the full support of the late Dr. W. H. Hatfield, F.R.S., and the late Dr. T. Swinden, I was asked to accept the Chairmanship of this Sub-Committee.

It is difficult to re-create now the atmosphere which prevailed at that time. We definitely felt that this test might provide a short cut which would help us in our fight to save ferro-alloys. Ships were going down laden with expensive ferro-alloys which could not in all cases be replaced, and time was an important factor, so that any short cut which held promise acquired what would

otherwise have been a quite undue importance.

This discussion has been planned round the headings in Section VIII of the Symposium. The twelve conclusions contained in this section fall readily into four groups, each of which will be briefly introduced by a separate speaker, who will discuss the conclusions, enlarge on their significance, and point out the pertinent features of the papers on which they are based. After each introduction, discussion will be invited on the conclusions and the individual papers relevant to them. By this method the Sub-Committee hopes to obtain the advantage of a discussion on the widest possible basis, with the benefit of new ideas and points of view to assist it in any future work.

In addition to the acknowledgments contained in the Report itself, a tribute is due to Mr. Headlam-Morley and Mr. Chattin for the very heavy editorial work which they have put into this Symposium. They have had to contend with gigantic difficulties in obtaining paper and arranging for the printing. Now that this volume has appeared, I am sure that there will be general agreement that it is up to the highest standards which are associated with these Special Reports.

Mr. **C. R. Wheeler** (Chairman of the Mission to North America in 1943) : Mr. Oliver has indicated my personal interest in this matter, as in a measure I was one of the parents of the project. First of all, I should like to congratulate Mr. Oliver and the Sub-Committee on the excellent work which they have done. Mr. Oliver referred to the mission, of which I was Chairman, which went to America during the war. I think that to have a proper appreciation of this Symposium it is important to understand the background which existed at that time. As Mr. Oliver has already said, a severe shortage of certain alloying materials was being felt both by America and by ourselves, partly on account of the shipping difficulties of the time and partly owing to an actual loss of sources of supply.

As is natural in times of stress, there was a feeling on the part of some people here that the industries in America were being perhaps rather more wasteful than they should be ; on the other hand, there was no doubt a feeling on the part of certain people in America that the industry in England was also being a little more wasteful than might be. Accordingly, it was decided that a party should go and discuss with the Americans exactly what we were doing in the whole range of war production as applied to alloy steels.

I think that it is worth recalling who were the members of that team, as they were all, with the exception of Mr. Burn, members of the Institute. There were first of all the two very well-known members of the Council of the Institute whose deaths we all lament, Dr. W. H. Hatfield and Dr. T. Swinden. The other members of the party were Mr. W. Barr, Mr. H. H. Burton, Mr. D. A. Oliver, Mr. F. Saniter, and Major Senior.

Remarkable success was achieved in the spirit in which the discussions were held. Naturally there was controversy, and this particular subject of hardenability, I may say, produced an active degree of controversy. Notwithstanding the different points of view, a joint report was produced, and I think it worth while to quote from this report, a section of which had the effect of giving birth to this Symposium. Under the heading of " Recommendations," the report said : " That the extended study be continued in Britain to correlate hardenability with physical properties."

The broad conclusions of this joint Anglo-American team were that America had gone farther in conservation by means of the analysis of alloy steels as applied to the war effort. The NE steels as a whole contained less alloying elements than the British equivalent, the En steels. On the other hand, on careful examination we found that other methods of conservation of alloys as applied in this country were at the time more effective than those applied in America, with the main result that, taking the actual usage of ferro-alloys to achieve a given requirement, it was broadly " as you were."

The main point to be settled between the two countries was how effective were the measures applied. You are all aware of the very great importance attached at that time in America to hardenability and the Jominy test. The Americans were keener on cutting down in terms of the final specification of alloys in a given specification than we were. However, the result of the somewhat lengthy discussions was a very high degree of unanimity in approach.

Some of our British friends were sceptical of the tendency at that time in America to make hardenability the answer to all problems of specification, and what we now see in front of us is the careful analysis of the matter which has been made by the Sub-Committee under Mr. Oliver. I think that I am right in saying—and this is a matter of some interest—that the almost universal opinion of the mission at the time was that while there was a variety of viewpoints—from Mr. Oliver, at one end of the scale, who held that there was a great deal to be gained by the use of hardenability tests, to the rather die-hard point of view of Dr. Hatfield, at the other end of the scale, who felt that hardenability was a toy which would have but little practical result—the whole matter deserved careful investigation. It will be a matter of considerable interest to me to discover what is the conclusion at which

you arrive today. I am not looking for a final conclusion, but the consensus of opinion.

This work which has been done and this discussion today form another link in Anglo-American metallurgical co-operation, which was perhaps one of the most decisive factors, if not the most decisive, in the defeat of the Axis. We all know what benefits are obtained from the free interchange of opinions and views, particularly on a practical basis.

I wish to conclude by recording a real sense of gratitude to the Institute on the part of those of us who feel strongly the need for the maximum of international, and particularly Anglo-American, co-operation in the metallurgical field.

The **President** (Dr. C. H. Desch, F.R.S.): We are indebted to Mr. Wheeler for his account of how this work arose. We now come to the discussion of the separate sections of the Symposium, and I will ask Dr. W. Steven to introduce the first of these sections.

Discussion on
REPRODUCIBILITY OF END-QUENCH HARDENABILITY CURVES

Dr. **W. Steven** (Messrs. William Jessop and Sons, Ltd., Sheffield) introduced the discussion, based on Conclusions (1) to (6), pages 413 to 414, of the Symposium, and summarized the information given in the relevant sections of the Report.

DISCUSSION

Mr. **T. M. Service** (Messrs. Wm. Beardmore and Co., Ltd., Glasgow): The hardenability of steel is rather a wide subject. There seems to me to be a tendency to base the whole treatment of steel on the question of hardenability, and I wonder whether that is really correct. In Section IX, page 418, Mr. Oliver sums up by saying :

> "The information obtained indicated that large differences in hardenability may arise within a single cast. It is therefore necessary to conclude that no single end-quench hardenability curve can be regarded as sufficiently representative of the hardenability of a given cast or ingot, or even, in some cases, of a single bar of steel."

If that is the case, that hardenability is something which you cannot tie down to any particular test, is that the right method of going about the work ?

It seems to me, in reading through all these papers, that there are one or two points which have never been investigated. First of all, we are told that a commercial steel was used. Are we satisfied that the commercial steel which we are using is the best for the purpose of a scientific investigation ? I would suggest that the Sub-Committee should investigate the quality of the steel which they have been using.

The second point is the temperature for rolling. Were all these bars rolled at the same temperature, or were there differences in the temperature of rolling ? Anyone with a large experience of steel manufacture knows that any heat-treatment—and heating for rolling is part of the heat-treatment—has some influence on the ultimate heat-treatment of these steels, and probably if one got down to it one would find that some of the discrepancies which have been discovered in the hardenability could be washed out if a uniform steel and uniform heating for rolling were used.

Another point which it seems to me that the Sub-Committee have not taken into account is that no time is given for the mass effect. They have just given it a certain time, but they do not say what. They have heated to 850° C. or 870° C., or whatever it may be, and then hardened, but what about the change which takes place in the inside of a mass ? It is all right for a 1-in. bar, but when you come to a 4-in., 8-in. or 24-in. bar, what happens then in the inside of the bar ?

I think that Mr. Glen in his paper says that in a heat-treated steel one has to meet certain specified mechanical tests with a minimum Izod figure. The hardenability test does not tell you that you are attaining the desired figure.

It appears that these papers are all concerned with steels up to about 4-in. round or 4-in. square, but many of us have to deal with very much heavier masses than that, in fact, anything from 40 to 60 in. in dia. This Report does not give any indication of the time for which we should keep the material at the hardening temperature in order to have the centre of the mass equivalent to the outside. Most of us find that tests taken at the centre of a large mass do not give the same results as those taken on the outside, though you can get very excellent tests on the outside.

One question which seems to have been neglected is that of tempering after hardening. Most steels have a range which is called the " fibre range." No indication is given in the testing for mechanical qualities of what the fibre range is in the particular steel investigated. That can easily be found out by experiment, and most people who have to heat-treat steel determine this fibre range and treat the steel within that range. If a bar is heated to 870° C. at one end and to 300° C. at the other end, when the bar is broken, the range within which the steel will be fibrous can be found, and that is the kind of steel that the

engineer wants. He wants to ensure that his material is in the toughest possible condition, and I do not think that we are going to get that from hardenability curves alone.

Mr. **E. H. Bucknall** (The Mond Nickel Co., Ltd., Birmingham) : In view of Mr. Service's remarks, I feel that it is desirable to emphasize the status of the hardenability test as carried out according to the procedure recommended in Section VE of the Symposium as a means of proving or disproving the uniformity of a billet or other piece of steel across the section. Since the completion of the work described in Section VE, that method of investigation has been used in The Mond Nickel Company's laboratories in the investigation of the variation in hardenability of a range of British En and American NE steels. The examination of these steels has amply confirmed the impression given in Section VE that variation in hardenability occurs to an important extent in commercial steels purchased in the ordinary way.

Of twenty-six British En steel billets examined recently, only fourteen were approximately uniform in hardenability and etching characteristics across the billet sections ; the remaining twelve billets all showed more or less prominent central billet squares of the sort indicated by Dr. Steven. Of those twelve, one had a more hardenable centre than outside, and the other eleven had softer centres than outsides, showing that the Jominy test as normally carried out is very liable to give an exaggerated view of the hardenability of a steel by showing you what the outside material would do if it were at the centre, and not showing you what the centre material does when it is at the centre of a large section.

As with the steels referred to in Section VE of the Symposium, none of these twenty-six steels showed any detectable non-uniform distribution of sulphur, carbon, manganese, nickel, chromium, or molybdenum across the billet section, so that the origin of the variation must be sought elsewhere. Two instances were found of billets supplied as being from the same ingot which differed considerably in the pattern of hardenability across the section, showing that, as suggested in the Symposium, longitudinal as well as transverse variations can be expected.

These observations encourage the idea that it is now an urgent matter to examine by hardenability tests, and possibly also by transformation measurements, the product of ingots to check how far the variation found in billets is due to initial differences in the ingot. An examination might be carried out on steels of high and low hardenability, in each case using steels which are prone to segregation or to variation in hardenability, and steels which have the reputation of being relatively free from such troubles. As suggested in Section VE, such work could very well be carried out by examination of test-bars taken to represent the standard positions used in the work of the Heterogeneity Committee in their thorough survey of variations of composition in steel ingots.

Mr. **G. Meikle** (Royal Aircraft Establishment, Farnborough) : I should like to suggest that quite a number of the variations found for which no explanation is given may be connected in some way with retained austenite. That problem, I think, has not been investigated by the Sub-Committee. In the paper by Pumphrey and Jones, for instance, it is suggested that the difference in hardenability between two similar steels, varying only in carbon, is due to the size of the carbide particles out of solution ; but I think that a simpler explanation would be that the higher carbon steel gives more retained austenite, and therefore one gets an apparent lower hardenability. I suggest, therefore, that these two steels might be re-examined after a sub-zero treatment, and the same might be applied to a number of the other higher-alloy steels in the series.

Dr. **J. H. Whiteley** (Consett Iron Co., Ltd., Consett, Co. Durham) : Judging from the contents of the papers comprising this Report, the main effort of the Sub-Committee has been to ascertain whether the Jominy test could be relied on as a guide in quenching practice, and I have nothing but praise for the work which has been done ; from first to last I think it is excellent. Although the test is a very simple one it gives rise to a number of difficult problems, so that the interpretation of the results is in many cases by no means easy. As is pointed out by Mr. Glen in Section VIIA, for each type of steel a knowledge of the S-curve should be a valuable aid since the form of the hardenability curve must depend upon the transformations which can occur, and their speed, at different temperatures below Ar_1. To illustrate this I would draw attention to the way data provided by the S-curve may be used to explain a feature of the Jominy curves for plain medium-carbon steels which is shown in greater or less degree by the curves of all those mentioned in the Report.

According to an atlas of S-curves published some years ago by the United States Steel Corporation, transformation of austenite in 0·50% carbon steel at temperatures above 300° C. occurs at the quickest rate in the proximity of 550° C. where it takes about 5 sec. to complete in pieces quenched in a bath at that temperature. As the quenching temperature is lowered the speed of the transformation gradually diminishes until at about 350° C. a period of 5–10 sec. can elapse before the austenite begins to change, and thereafter the progress of the transformation is much slower than

at 550° C. To make use of this information in studying the Jominy curve it is, of course, necessary to know the rates of cooling at different points along the bar, and fortunately that requisite is supplied by the excellent work of Russell and Williamson in Section IVB. The data they give show that a point $\frac{1}{8}$ in. from the quenched end passes through the range 650–450° C. in about 2 sec., so that the transformation of the austenite in a 0·40% carbon steel should not there be completed. At a position $\frac{1}{4}$ in. along the bar, however, no austenite should remain at 450° C. since the transit through the above range there takes about 8 sec. Hence retained austenite should be present in the bar up to a point somewhere between $\frac{1}{8}$ and $\frac{1}{4}$ in. from the quenched end when it reaches 450° C., with the result that a retardation of the softening process must occur.

Such an effect is certainly apparent in all the Jominy curves of plain medium-carbon steels given in the Report, and the interesting thing is that the hardenability increases with the degree of the initial retardation. This is well illustrated by the two 0·40% carbon steels, Nos. 1 and 12, of Section IIA. Although these two steels are very similar in composition, the hardenability of steel No. 12 is definitely greater than that of steel No. 1, and a comparison of their Jominy curves up to 0·2 in. in Figs. 1 and 6 will show that the retardation in steel No. 12 was decidedly more prolonged. Other examples showing the same feature are to be seen in Fig. 69. Thus it would appear that the hardenability of a medium-carbon steel is connected in some way with the rate at which the retained austenite transforms, as is undoubtedly the case with alloy steels.

Mr. **H. H. Burton** (English Steel Corporation, Sheffield): Dr. Steven referred several times to surface material as generally showing greater hardenability than the inside material. I suggest that the term "surface material" is hardly correct; it seems from the diagrams that it is more probably the columnar material. I do not know whether it was done regularly, but I should like to know whether the specimens which were cut from these billets which showed these variations in the two positions which he indicated were macro-etched longitudinally, in order to show whether or not that is a fact. It is not unreasonable to suppose that the columnar material may show much greater uniformity; it is more uniform in other ways than the so-called equi-axed material inside, and it is reasonable to suppose also that it may have better hardenability properties.

Dr. Steven also referred to the difference between different ingots in the same cast, and even different pieces of bar. I wonder whether these differences were always related—probably they were not—to the position of the said bar or billet in the ingot, and also whether the possible effect of different amounts of hot-working reduction was taken into account. I wonder also whether all the bars were produced by a uniform method; were they all rolled, or were some of them forged? From the experience that a number of us had during the war and at other times with gun steels, it would not be surprising to find that a billet from the same ingot might show different results according to whether it was produced in its final form by forging or by rolling.

There is a great deal of evidence on that point. For example, in making a gun forging, the results were always better by forging the billet used for the final forging than by rolling it. It is rather surprising. Not only were the transverse properties rather better, but in many cases after similar heat-treatment the tensile strength was some 2 tons/sq. in. higher. That is, of course, after tempering, but it would appear to indicate rather greater hardenability.

Dr. **F. W. Jones** (Brown-Firth Research Laboratories, Sheffield): With regard to retained austenite, a question which has been raised by previous speakers, and particularly with regard to the steels investigated in the work described in the paper by Mr. Pumphrey and myself, I feel that the only influence that retained austenite is likely to have on the Jominy hardenability curve will be in the hardness at the quenched end of the bar. Further along the Jominy bar the softening is due to the transformation from austenite to ferrite and carbide at a fairly high temperature, and that is largely what determines the hardenability of the material. Only near the quenched end of the bar where the steel remains austenitic down to low temperatures (below 250° C.) is retained austenite likely to have an appreciable effect on the hardness. Even there the effect is quite small, an increase in hardness from about 900 to 930 D.P.H. being obtainable by a treatment in liquid air.

Dr. **W. J. Wrażej** (Imperial College of Science and Technology, Royal School of Mines): A production metallurgist always endeavours to obtain approximate information about the steel which he is using. It is well known that different casts of steel with similar compositions are liable, when heat-treated, to show different mechanical properties, as has already been mentioned in this discussion. This has been demonstrated in examples of carbon tool steels and plain carbon constructional steels by F. Rapatz* and myself. Samples quenched at 780°, 830°, and 880° C. showed different depths of hardening. It was assumed that this was connected with different oxygen

* *Hutnik*, 1937, vol. 9, No. 1, pp. 5–12.

contents in the steels, since the chemical analysis* carried out by Dr. Diergartten and myself at Professor Oberhoffer's Institute in Aachen, in 1928, disclosed this. It is a great pity that the chemical analyses of the steels investigated in these researches are not complete with respect to the determination of gases in the steels. I hope that this can yet be done in order to complete the examination, since such data are of great importance. The data collected in this Symposium should, with further exploration, prove of even greater benefit.

In my opinion, the reasonable prediction of the behaviour of steel during heat-treatment should really be based on the chemical analysis, when such complete analysis can easily be obtained, and on the nature of the atomic arrangement in quenched steels. Some recent remarks of mine on the γ–α transformation† indicate that some of the outstanding problems in heat-treatment are not far from elucidation.

I appreciate very much the frank conclusions in Sections VIII and IX, and especially the closing sentence of Mr. D. A. Oliver that "Apart from problems of immediate practical import there are others of a more fundamental nature awaiting attack." I think it will enable us to forget such remarks as "that retained carbides enable the transformation of austenite to take place."

Mr. **R. J. Brown** (Nuffield Central Research Laboratories, Coventry): I should like to make what may be regarded as some critical comments on the Symposium. Details are included of the standard so-called Jominy test-specimen; the American standard Jominy test-specimen (*S.A.E. Handbook*, 1946) is produced from a wrought bar, either rolled or forged, whereas in the Symposium only the diameter of the bar is stated, without reference to the size or form of the material from which the specimen is produced, so that a very big variable is at once introduced into all the work which has been carried out.

The result of the work undertaken is undoubtedly extremely valuable in drawing attention to the variations which occur in large sections of steel, variations of which we were all reasonably well aware, and also, presumably, variations which the Americans were anxious to "iron out" in adopting a standard procedure for the Jominy test.

Another point is that the Jominy test was evolved, I believe, from the user angle, while the papers under discussion have been presented from the manufacturer's angle. I have no wish to be acrimonious, but as a user I feel that greater control of this property of hardenability, however it may be determined, is essential. In the United States the tendency before the war was to restrict the chemical-composition limits much more closely than in equivalent British specifications, but the adoption of the hardenability test must indicate that even close limits of composition were not sufficient to "iron out" the difficulties experienced in the bulk heat-treatment of steel components. I refer to bulk heat-treatment as we do not know it in this country.

I feel that some form of hardenability test, whether the Jominy or another, is essential. The practical heat-treater is probably more interested in the response of the surface layers of the steel than of the core, but the core has obviously to be taken into account, particularly in the case of a component of varying section machined from a fairly large piece of steel. The surface layers of the component, however, are, generally speaking, the stressed layers, and it is in those layers that we are most interested.

Reference has been made in this discussion to the quality of the steel used; it is therefore apparent that the acceptance in the U.S.A. of H specifications is based either upon a misconception on the part of the Americans that variations do not occur within either cast or ingot, or else upon the fact that their product possesses greater uniformity than that produced in this country.

Dr. **J. H. Whiteley**: In Section VIIB Mr. Pumphrey and Dr. Jones put forward the interesting suggestion that the degree of hardenability of a hypereutectoid chromium–molybdenum steel is connected with the size of the undissolved carbide particles at the moment of quenching, but the evidence they present does not quite satisfy me that such is the case. For instance, Fig. 375 shows a series of six Jominy curves obtained after successive quenchings at 825° C. These curves show that the hardenability increases after each quench so that the *D*5 position moves about 0·7 in. up the bar during the six treatments. Now, one would expect that these repeated quenchings would tend to diminish the sizes of the carbide particles but, if the authors' suggestion is correct, the opposite should be the case. In fact the particles should have become considerably larger, for even when the size of the particles increased from that shown in Fig. 365 to that seen in Fig. 367, the *D*5 position advanced only about 0·3 in. more than after the above six quenches. The authors' contention would therefore have been more convincing if they had given illustrations showing an unmistakable increase in the particle size from the first to the sixth quench.

* The steel unsusceptible to heat-treatment:
1·06% C, 0·23% Si, 0·21% Mn, 0·0043% O_2, 0·00015% H_2, 0·0089% N_2, 0·0032% O_2, 0·00014% H_2, 0·0093% N_2.

The steel susceptible to heat-treatment:
1·10% C, 0·23% Si, 0·22% Mn, 0·0091% O_2, 0·00005% H_2, 0·0013% N_2.

† *Nature*, 1944, vol. 153, p. 308.

Discussion on

THE INFLUENCE OF CHEMICAL COMPOSITION ON HARDENABILITY

Mr. **J. Glen** (Messrs. Colvilles, Ltd.) introduced the discussion, based on Conclusions (7) and (8), pages 414 to 415 of the Symposium, and summarized the information given in the relevant sections of the report.

DISCUSSION

Mr. **J. Woolman** (Brown-Firth Research Laboratories, Sheffield) : I should like first to congratulate Mr. Oliver and his Sub-Committee on the vast amount of work which they have carried out, and which has culminated in this excellent report. It is not, I think, possible to confine the present discussion entirely to the subject of the effect of composition on hardenability so that I hope I shall be excused if occasionally I refer briefly to other sections of the report.

Since Grossmann first attempted to assess the effect of composition on hardenability, it has become abundantly clear that the problem is not as simple as Grossmann indicated, owing to the fact that, as has been pointed out, the hardenability effect of one element may be appreciably affected by the presence of another. Grossmann himself, I believe, realized this when he adopted the idea of using mutiplying factors instead of additive factors, but it is clear that this is not adequate to deal with complex steels.

The paper by Mr. Glen, therefore, is to be particularly welcomed, since it emphasizes to a very marked degree the difficulty of assessing adequately the hardenability from the composition, even taking into account the influence of grain-size, the effect of which, as pointed out by the author, is itself an unknown quantity, since it undoubtedly is also affected by composition.

Apart from the mutual interaction effect of the different elements, there are several other factors which influence the hardenability, and which are not taken care of by the nominal composition and grain-size. There are, first, the pre-condition of the steel, as was shown by Pumphrey and Jones, as far as it affects the nature and condition of the carbides, and hence the extent and possibly the uniformity of solution of the carbides in the austenite at the hardening temperature ; secondly, the presence of residual elements, not usually analysed for, which may have a marked effect, and here I include the gaseous elements, oxygen and nitrogen, which have already been referred to ; and thirdly, the mode of crystallization in the ingot, a factor already mentioned by Mr. Burton. It would appear from the paper by Mr. Bucknall in Section Ve and from the paper in Section Vd that the mode of crystallization in the ingot has an appreciable effect, since it is shown that the departures from uniformity and hardenability cannot be accounted for by heterogeneity of composition alone.

I think that this effect really merits further investigation, since it is possibly a very large factor in the failure adequately to predict the centre hardness of quenched specimens from the Jominy test results. Indeed, in all this work we should not lose sight of the fact that the main application of hardenability data is in connection with the heat-treatment of bars and forgings ; and, since it is shown in the report that the relationship previously claimed between the hardness at a certain position of a Jominy bar and that at the centre of a quenched bar does not hold, any conclusions on the effect of composition based on Jominy tests cannot be considered as much more qualitative, especially if simple multiplying factors, as employed by Grossmann and by Crafts and Lamont, are used to assess the effects of composition.

It must be borne in mind that hardenabilities are usually measured in terms of the ideal critical diameter obtained by the use of one of several different assumptions from the Jominy test results (derived, for example, from the point of inflection of the hardness curve plotted on a log distance scale or on a linear distance scale, or from the half-hardness values, or from the position having half martensite structure, etc.) and these different methods of assessing the ideal critical diameter cannot all represent the conditions at the centre of the bar identically.

Mr. Glen has made a very valuable contribution to the subject, and is to be congratulated on the work which he has done. The results which he gives go a long way towards elucidating the influence of composition, but there are one or two points to be mentioned in connection with his experiments. First of all, they were carried out on synthetic high-frequency melts, and may therefore not give identical results to those with similar steels made in the electric-arc or Siemens furnaces. Again, all the steels were first air-cooled from 900° C., and all were given a standard austenizing temperature of 880° C. The air-cooling from 900° C. will have different effects on the state of the carbon in different steels according to their alloy content, and it is possible that some of the complexity of the author's resulting curves showing the effect of composition may be due to this cause. The temperature of

880° C. is rather high for a quenching temperature for many of the steels, so that again the results may not be applicable to ordinary materials hardened from the usual hardening temperature.

Dr. **N. P. Allen** (National Physical Laboratory, Teddington) : I think that many of the difficulties in assessing the effect of an alloying element upon hardenability arise from the fact that there is no such property of a steel as hardenability at all. The effect of an alloying element on hardenability is the resultant of the effect of the alloying element on all the reactions which may take place between the quenching temperature and the temperature at which martensite forms, and the effect of the alloying element upon the hardness of such reaction products as appear if the cooling is sufficiently slow.

We know that there are at least four different reactions which may occur in cooling from the quenching temperature to the martensite formation temperature. These are the formation of pro-eutectoid ferrite, the formation of pro-eutectoid cementite, the formation of pearlite, and the formation of the intermediate product, the so-called bainite. By studying these things in great detail even more reactions have been distinguished. It is quite certain that the effect of alloying elements upon each of these reactions is different. The outstanding example is that of molybdenum which has a striking effect on the formation of pearlite and a relatively less vigorous effect on the formation of the intermediate product.

In these circumstances it really is not to be expected that the effect of an alloying element shall be expressible in a single figure such as Grossmann's multiplying factor, and it is not surprising, therefore, that Mr. Glen finds that it is very difficult to assign a value. The idea of a single mutiplying factor is essentially wrong, and what we have to do is to retrace our steps and study separately the effect of the alloying elements on each of the reactions which are important in this connection. That will take us a very long time. Meanwhile, we shall find that Mr. Glen's paper is a very useful mine from which to obtain information on the general effects.

Mr. **T. M. Service** (Messrs. Wm. Beardmore and Co., Ltd., Glasgow) : Although I have read Mr. Glen's paper, I have not studied it sufficiently to be able to speak very learnedly about it. He has raised the question of the effect of alloying elements. I can go back to the time when there was no such thing as nickel steels in commerce. We started with nickel steel, and then went on to nickel–chromium steel, and then we had the Krupp armour plate, which contained 2% of chromium, 4% of nickel, and about 0·3% of carbon. Krupp based his formula, after he had evolved his steel, on the fact that a well heat-treated nickel–chromium steel always contains 0·6% of chromium in solution, not as carbide, as we are generally led to believe. He multiplied his 0·30% of carbon by 4·3 for the carbide and got 1·28, and to this he added 0·6, and so got 1·88% of chromium which he had to put into his steel. He was aiming, of course, at getting a steel which, no matter how you broke it, would always be fibrous, whether it was anything from 2 in. up to 18 in. My own experience is very much in line with that.

Mr. Glen, if he perseveres in the line he is taking, will probably give us a few more ideas on how we can conserve alloys. At the moment, for heavy plates we cannot get away from 4% of nickel and 2% of chromium. During the war, when alloys were scarce, we had to reduce the nickel and found that for plates up to 3 in. or 4 in. 0·6% gave as good results as the higher alloy, but above that we still have to come back to the old composition.

Dr. **L. K. Janiček** (Mining and Metallurgical Institute, Ostrava, Czechoslovakia) offered the following contribution on behalf of himself and Dr. **R. Pospíšil** (United Steelworks, Nt. Corp. Steelwork, Poldi) : We would like to make a few remarks concerning the influence of certain steel-making conditions on hardenability, taking as an example a steel produced at Poldi Steelworks, Kladno, which has the following limits of chemical composition :

Carbon	0·38 — 0·45%.
Manganese	1·60 — 1·90%.
Silicon	< 0·40%.
Vanadium	0·10 — 0·18%.

The nickel and chromium contents were not specified but fell within the following limits :

Nickel	0·10 — 0·20%.
Chromium	0·15 — 0·75%.

Approximately 800 heats of this steel were made in four furnaces of different type, namely, (*a*) basic open-hearth furnace *M*, (*b*) 6-ton basic electric-arc furnace *F*, (*c*) 20-ton basic electric-arc furnace *S*, and (*d*) Kjellin low-frequency furnace *C*.

The duplex process was mainly used in (*b*), (*c*), and (*d*), the steel being melted in the open-hearth furnace and refined in one of the above-mentioned furnaces.

The Jominy test was used for the control of hardenability, which was given as the distance in millimetres from the quenched end of the specimen to the point showing a hardness of 50 Rc. It should also be stated that the Jominy test-bar was always prepared in the same way, *i.e.*, from ingots of equal size under identical conditions of rolling and forging.

In spite of the fact that different formulæ

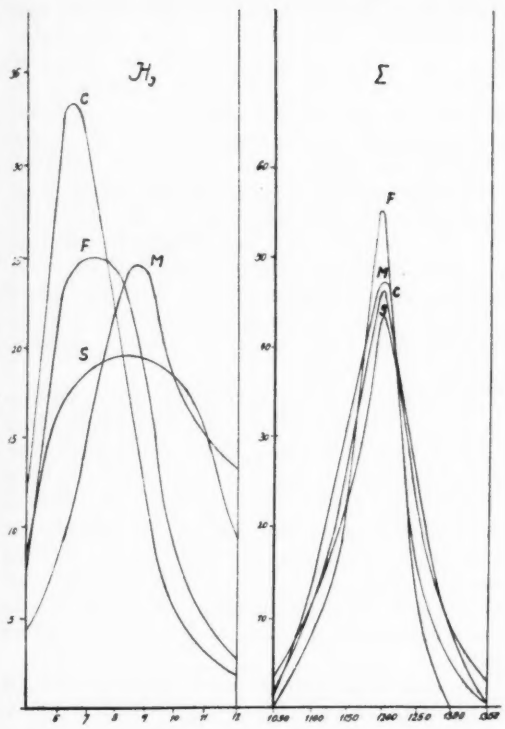


FIG. A—Frequency curves of Σ and of hardenability (*see* L. K. Janiček and R. Popišil's contribution)

according to which the hardenability is an additive property have not such a general validity as was admitted a short time ago, we came to the conclusion that they could be used if the contents of elements controlling the hardenability varied within narrow limits.

For the above-mentioned steel the following formula can be used :

$$\Sigma = 1000\,C + 400\,Mn + 500\,Cr + 100\,Ni$$

As Fig. A shows, the frequency curves of Σ are the same for the steel made in different furnaces. In spite of this, however, the frequency curves of hardenability are not the same. It is evident that, besides the chemical composition, the hardenability is affected by the metallurgical process characterized by the type and the size of the furnace used. The differences are statistically significant and are of different importance for the practice.

When the hardenability was studied as a function of Σ, the influence of different furnaces was also found. The hardenability rose most rapidly for the open-hearth furnace *M* and sucessively less for the furnaces *S*, *F*, and *C*.

According to our opinion it is of interest to analyse the stated influence of the metallurgical process on the hardenability. In the given case the Jominy test has been very useful for this purpose because of the appropriate composition of the produced steel.

Another question is : What is the general significance of this test and where are the limits of its use ?

Mr. **H. Allsop** (Brown, Bayley's Steel Works, Ltd.) : Mr. Woolman referred to the possible effect of ingot structure on hardenability. We have done some preliminary work in this direction since the matter was submitted for this Symposium, and there does appear to be a connection between the macrostructure in the billets and the limits of the zone of low hardenability in the centre ; but, to revert to what Mr. Burton said earlier, this further work does not indicate that the low-hardenability zone coincides with the limits of the columnar material.

There are several reasons why it would appear that the columnar zone does not wholly represent the high-hardenability zone. First, in the ordinary way the columnar crystals are longer at the bottom of an ingot than at the top. We have investigated the low-hardenability area of the top, middle, and bottom bars of the three ingots used in the last section of the paper by Dr. Steven and myself, and in point of fact there is no low-hardenability area in the sections from the top ; there is an extensive area in the middle sections, and a larger area at the bottom. That is the reverse of the normal depth variation in columnar crystals.

Secondly, the low-hardenability area both in those billets which we examined and in the examples shown in Bucknall's work lies much deeper than the normal columnar depth in an ordinary ingot. Thirdly, on many of the macrostructures which we have examined the limits of the columnar zone in cross-section are quite clearly seen ; inside that there is a free crystal area, and well inside that is the low-hardenability area.

Mr. **E. H. Bucknall** (The Mond Nickel Co., Ltd., Birmingham) : Mr. Glen has shown that the situation as regards the contribution of alloying elements to hardenability is considerably more complex than was indicated by Grossmann, but it does seem to me that Mr. Glen nevertheless clings to some of Grossmann's ideas to a greater extent than I would feel to be justified. One instance which struck me particularly in his opening remarks this morning was his emphasis on the ideal critical diameter being dependent on 50% martensite hardening. There is nothing very wonderful about 50% martensite hardening ; in an alloy steel it constitutes no guarantee whatever of adequate properties being achieved after tempering. I believe that 50% martensite hardening was chosen by Grossmann as a criterion simply

because in plain carbon steels the fall both of hardness traverse curves across billets and of Jominy curves is sudden and the 50% martensite point defines the step from 100% of martensite to 0% of martensite. In most alloy steels the 50% martensite point does not have this significance, because the Jominy curve falls with a shallower slope.

I was particularly interested to compare the typical curve given by Mr. Glen for the effect of manganese when acting alone, with that given by Brophy and Miller in a recent paper for the effect of nickel when acting alone, because these two elements produce very similar S-curves in steels, and are therefore as worth comparing on a numerical basis as any two elements could be. Mr. Glen shows an approximately linear relation of hardenability with composition for manganese; Brophy and Miller, on the other hand, showed a steeply concave curve for the effect of nickel, the effectiveness of nickel being low up to 2% but increasing rapidly with further additions, so that with 5% of nickel they showed in their paper an ideal critical diameter of 5 in.

The characteristic effects of the elements do seem to be a serious matter from the point of view of the prediction of hardenability, and particularly from the point of view of the justification of multi-alloy steels on the basis of hardenability calculations. It seems to me that Grossmann's calculations favour the multi-alloy compositions because he used an additive principle for successive additions of a single element and a multiplying principle for additions of different elements. A steeply concave curve, on the other hand, indicates a multiplying principle for progressive additions of a single element, and would seem to weaken the argument on hardenability-calculation grounds for multi-alloy steels.

In our laboratories parallel determinations of isothermal-transformation characteristics and Jominy curves have convinced us that the isothermal transformation characteristics can be regarded as a more precise and discerning hardenability measurement, and one which expands the range of testing far beyond that with which the Jominy test can cope. I think that Mr. Glen will be interested to hear that we wasted at one period a good deal of time trying to systematize on a calculation basis the contribution of alloying elements to transformation characteristics. When we were expressing the effect of elements on transformation by means of the maximum transformation velocity, which is the velocity of a first-order reaction corresponding to the fastest transformation shown in the steel, and dealing with single additions, there seemed often to be a simple rule that each progressive addition of, let us say, 0·1% of an alloying element reduced the transformation velocity by a constant factor. The factor was approximately 0·4 for carbon, 0·76 for manganese, and so on. Efforts to calculate the behaviour of complex steels on this basis, even when they are restricted, as Dr. Allen suggested this morning that they should be, to steels which had similar transformation habits, and even when confined to the intermediate transformation range, were unsuccessful, though we went so far as to say "Let us assume that an individual element may have different effects according to what other elements are present, but that the effect is constant in steels which already have a given transformation velocity in the absence of that addition."

This failure was not surprising, for a variety of reasons. The first reason has been emphasized by Dr. Allen this morning, but I would go further than Dr. Allen on the question of the division of the sorts of transformation from this point of view. Within a single type of transformation as generally recognized we find some alloying elements contributing by reducing the number of centres of transformation, and others acting more strongly on the rate of growth from those nuclei.

The second point is that although within limited composition ranges there seemed to be this multiplying-factor effect for the individual alloying elements, this rule is an over-simplification of the issue where wider ranges are concerned. In the case of carbon additions to a variety of alloy-steel bases, we have found that so far as both the intermediate transformation and pearlitic transformation are concerned, there is a maximum effect of carbon at some intermediate composition, which in the cases of pearlitic transformations is often near the eutectoid composition, but tends to be different as between the intermediate and the pearlitic transformation. It would appear that much more insight into isothermal-transformation mechanism, and in particular into the relation between isothermal-transformation conditions and continuous-cooling conditions, is needed before the effect of alloying elements can possibly be put on a rational basis.

A further point concerns Mr. Glen's observations as to the effect of additions of boron and aluminium on the hardenability of alloy steels. He showed, as we have found in our own laboratories, large effects of such additions, but I feel that the question should be raised of what steps were taken to ensure representative results for the boron-treated steels. Despite some American views to the contrary, our own experience has been that boron-treated steels are particularly prone to hardenability variations across the section. For instance, in tests on eight pairs of commercial billets representing different NE

types with and without boron treatment, in all the steels containing less than 0·4% of carbon the boron-containing member of a pair showed a much more pronounced billet-square and much more hardenability variation across the section; while the boron treatment had usually doubled the distance along the " outside " Jominy bar for a given fall in hardness from the quenched end. There was usually only a smaller effect, say, a 50% increase at the centre.

Those results might, of course, be interpreted as a basis for the rational use of boron in alloy steels, but I think their main significance is to show that minor additions may play a very large part in affecting the hardenability and distribution of hardenability within the product of an ingot. Boron is known to exercise a large effect on the mechanism of transformation in steels ; but one thinks of boron as only one of a considerable number of elements which may either be introduced intentionally in steel-making or creep in to varying extents, and which have significant effects on hardenability *via* their effects on transformation characteristics.

Dr. **R. Genders** (Ministry of Supply) : After adding my congratulations to the Hardenability Sub-Committee for this excellent mass of work, and saying that all the members of the late Technical Advisory Committee have always felt that when it appeared, it would be found to have been well worth waiting for, I should like to refer to Dr. Allen's remarks and say a word on the other side, in favour of Grossmann, whose work was a valuable first exploration of this interesting field of multi-alloy steels. I am rather an admirer of the American habit of becoming very enthusiastic about a new method of approach to a subject and of over-simplifying it in the endeavour to " get it across " and to interest the widest possible circle of people.

I think we can say that had Grossmann not taken a short cut which has not proved to be the best route, we might not have been here today, and anyway it has resulted in a large number of people following him and so gaining an enlarged view of the subject.

Another general point which emerges from this Symposium is that there is an enormous field for further work, and particularly in what I regard as a rather more suitable field for the application of hardenability experimentation, that is, in the field of steels which are used in the near-completely-hard condition, and especially in the tool-steel field. I feel certain that in that field we shall obtain very valuable progress indeed, far more so than in this present field of engineering steels, which are, after all, considerably tempered after hardening, with the result that the gradient of properties is much flattened out and the effects of variations in hardenability proportionately lost.

Before leaving that point I must say that we ought to be very grateful for the early work of Jominy, which was a brilliant attempt to obtain in a simple manner results such as we had been obtaining by a very much more lengthy type of experimental work in the past. The reasons for embarking on the engineering steels are well known, but in spite of the limitations revealed by the present work, there appears to be a valuable scope for application of the test in properly selected fields.

For many years, of course, we have been interested in this subject of " mass effect," as we have called it. We have now come to use the American term " hardenability." I am not sure which is the better term. It is gratifying to know, from the results of Crafts and Lamont and other workers, that as a steel industry in this country we have been quantitatively on the right lines in the data which we issued in connection with specifications.

With regard to the effect of composition in general, it seems clear that carbon is a very important element. That was brought out by a good deal of practical work during the war, when we came to realize that free cementite or free ferrite acted as centres of transformation and therefore affected the apparent hardenability. It seemed fairly clear that the eutectoid composition had the highest hardenability. The whole question, of course, is one of complex constitutional aspects, and a great deal of work is required to elucidate it further ; but the curves shown by Mr. Bucknall have illustrated one very interesting line of attack.

Finally, I think it should be mentioned that during the inception of this work the Technical Advisory Committee owed a great deal to the co-operation of Mr. Howard Biers, who came to this country and discussed the matter with us, and who was also the means by which Crafts and Lamont undertook their very interesting work on British steels.

Mr. **W. E. Bardgett** (The United Steel Companies, Ltd., Stocksbridge, near Sheffield) : Oxygen has been referred to by one or two speakers, and it may be of interest to mention that in the early days of our experimental work in this field we encountered two samples of bar from one particular ingot which showed a pronounced difference in hardenability. We set out to find the cause of this difference, and in so doing carried out determinations of oxygen content. These showed no appreciable variation in the oxygen content of the two samples.

Discussion on THE PREDICTION OF HARDNESS AND TRANSFORMATION RANGES FROM END-QUENCH HARDENABILITY CURVES

PROCEEDINGS OF THE AFTERNOON SESSION : 2.15 P.M. TO 5.00 P.M.

Mr. **T. F. Russell** (English Steel Corporation, Ltd.) introduced the discussion, based on Conclusions (9), (10), and (12), pages 415 to 416 of the Symposium, and summarized the information given in the relevant sections of the report.

DISCUSSION

Dr. **N. P. Allen** (National Physical Laboratory, Teddington): We should begin by expressing our very great appreciation of the enormous amount of good work that the Sub-Committee has done. It is an open secret that Mr. Russell has been the mathematical adviser of the Sub-Committee on this subject, and he in particular has taken the subject to heart, worked on it in great detail, and done an enormous amount of calculation. It would not be appropriate to let this occasion go by without expressing appreciation of his devoted activities in this respect.

I propose to devote my remarks to the proposition that, provided that we take the variations of hardenability across the section into account, we can predict the hardness which will exist in a quenched bar, and I shall act somewhat as " devil's advocate " to see to what extent this conclusion can be broken down. We can express the most entire confidence in all the calculations ; what we have to examine is the assumptions behind those calculations.

In the first place, we have to assume that if in two pieces of the same kind of steel the cooling rates are the same at two points, the hardness and general properties will be the same ; and then we determine experimentally the rate of cooling along a Jominy bar of the steel and calculate the rate of cooling at a point inside another quenched bar of the same steel in which we are interested. The real problem is to calculate the rate of cooling inside an unknown bar, that is, inside any particular bar of an unknown steel.

The calculations are complicated, involving a knowledge of three things : The rate of heat transfer across the surface of the steel, the specific heat of the steel at every point (and with the specific heat we have to include any latent heat which is given out), and the thermal conductivity of the steel at every point ; but the characteristics of the material vary from point to point through the steel, and they vary from time to time during the quench. Furthermore, the specific heat and the thermal conductivity vary if transformation takes place in the steel in the process of quenching, and consequently we must know something about the transformation behaviour before we can carry out the calculation in a rigid way.

When we take these factors into account the problem becomes so complicated that it is quite impossible to deal with it, and therefore we simplify it by means of a standard procedure involving firstly the use of a reference steel to measure the " quenching intensity " of the quenching medium, and secondly the use of the " quenching intensity " figure to estimate the rate of cooling of the unknown steel.

The standard procedure depends on certain fundamental assumptions of which the validity has to be examined. The first assumption is that a quenching intensity can be assigned to a quenching medium which is, within limits, the same for every steel and the same for every size of bar. The second is that the half-temperature time is an adequate statement of the cooling rate at the point in question. The third is that the effective specific heat of the steel is constant, and the fourth is that the effective thermal conductivity of the steel is constant.

Taking those assumptions in order, with regard to the quenching intensity of the medium it is very noticeable that the workers have had the greatest difficulty in assigning a quenching-intensity figure to any particular quenching oil. The data lead to a suspicion that the quenching intensity of an oil is not a constant, but is a function of the hardening capacity of the steel and of the size of the bar.

In this connection, I want to refer, briefly, to some work by Dr. Jones and Mr. Pumphrey which is shortly to be published. They have measured quenching intensities directly, and find in certain cases that the effective quenching intensity varies according to the temperature of the steel and also according to the size of the bar being quenched. That confirms the general impression left by the difficulties which the investigators have had in fixing quenching intensities, so that there is a real doubt whether the quenching intensity is, in fact, constant.

With regard to the second point, the validity of the half-temperature time as a criterion of cooling rate, we are in a much happier position,

because here we know quite definitely that we are wrong. The paper by Jones and Pumphrey shows that the majority of the steels of the types in which we are interested transform during a quenching operation at temperatures below the half-temperature point ; and studies of isothermal transformations on various alloy steels would indicate that this is the general state of affairs. This correlation, therefore, ought to be based on a rate of cooling measured in an appreciably lower temperature range than is used at present.

With regard to the thermal conductivity of the steel, there are variations, but only of a minor character ; but with regard to the latent heat I think that it is important to point out that the evolution of the latent heat does alter the rate of cooling of the steel in quite a substantial way. Figure 42 of the Symposium demonstrates that, and it should be noted that the evolution of latent heat retards the cooling just at the point where retardation is important, *i.e.*, in the temperature range in which transformation takes place.

From a theoretical point of view, therefore, there are quite a number of objections to the mode of correlation that has been worked out, in view of which it is surprising that the correlation is as good as in fact it is. I think this is the consequence of two things. In the first place, there is a certain spuriousness (if I may say so without offence) about this correlation, inasmuch as one is generally comparing the behaviour of steels which are very much alike, and a number of the errors in the method are the same for the reference steel as for the steel being studied.

Another point of importance is that except when the steel is just on the balance point between hardening and not hardening, the hardness of the average steel is not extraordinarily sensitive to the precise rate of cooling. The greater part of the Jominy curve is made up of two relatively flat portions joined by a steep portion and if the conditions correspond to the flat portions the errors in the calculation of the rate of cooling do not matter very much.

When all has been done with regard to correcting for the variations of hardenability throughout the section, we find that the ultimate correlation is really not very perfect. The ultimate comparison is shown in a comparison between Fig. 236 and Fig. 239, and in the series of Figs. 209 to 230. Two points may be mentioned. First, the effect of the local variations in the hardenability of the steel was not in practice quite as severe as it would be calculated to be, and secondly the correlations in Figs. 209 to 230 are apt to be incorrect in the shallow-hardening steels in the small sections (*e.g.*, Fig. 209), whereas in the deep-hardening steels the correlations are apt to fail in the large sections. This is probably attributable to the effect of the latent heat of transformation on the rate of cooling.

If the transformation is prevented in a small bar of a shallow-hardening steel at the surface, the fact that the latent heat of transformation is not emitted will tend to increase the rate of cooling at the centre. Conversely, in a deep-hardening steel in a large section, if the latent heat of transformation once begins to be evolved the effect will be to retard the general cooling of the steel and to emphasize the difference between a steel which hardens and a steel which does not harden. Similarly, when deep-hardening and shallow-hardening areas are present together in the same steel, the evolution of heat from the shallow-hardening sections slows the cooling of the deep-hardening sections and encourages them to transform.

I do not wish to be understood to minimize the importance of the Jominy test or its usefulness, but feel that there is a case against over-elaboration of the mathematical side of the problem. The virtue of the Jominy test is really its simplicity and a comparison can be made between the Jominy test and the Brinell hardness test. We know that there is a correlation between Brinell hardness and tensile strength, which is very useful to the ordinary working metallurgist, but if the correlation is examined in detail it will be found that it is not exact, that it depends on the size of the ball, on the load, on the nature of the steel, and on a number of other factors which can with great pains be elucidated. In practice, however, people do not worry about that ; they simply use the Brinell hardness test and the correlation, and find out by experience just how far the correlation can be trusted and where they have to be careful. They find that they can make many deductions from the Brinell hardness of a material about its general properties, provided that the metallurgy of the material is sufficiently well known. It will be much the same with the Jominy test. We shall use it, and use such rough correlations as appear suitable by rule-of-thumb methods, and get on a great deal faster that way, and be on sounder ground, than if we attempt to calculate too far beyond the limits of our exact knowledge of the material.

Mr. **E. H. Bucknall** (The Mond Nickel Co., Ltd., Birmingham) : After listening to Mr. Russell's rather cautious presentation, and to Dr. Allen in the rôle of *advocatus diaboli*, we may perhaps be forgiven if we form the impression that the only absolutely sound use of the Jominy test is as a workshop tool for deciding whether one test-bar has an equal or a greater or a smaller hardened length than another steel when end-quenched. If we go beyond this simple statement

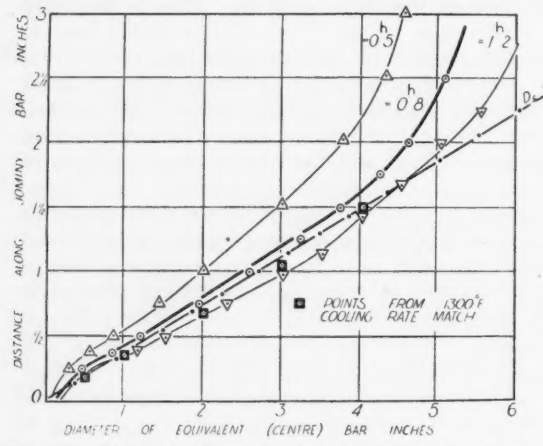

DISTANCE ALONG JOMINY BAR INCHES
DIAMETER OF EQUIVALENT (CENTRE) BAR INCHES
h = 0·5
h = 0·8
h = 1·2
D = 2·7 · J
POINTS FROM 1300°F COOLING RATE MATCH

FIG. B—Distance along Jominy bar plotted against diameter of equivalent (centre) bar (*see* E. H. Bucknall's contribution)

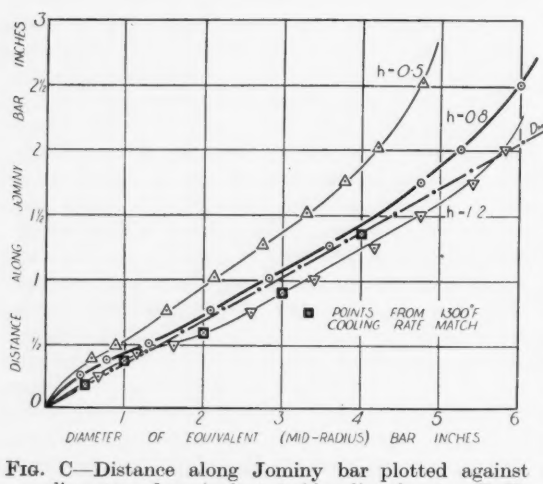

DISTANCE ALONG JOMINY BAR INCHES
DIAMETER OF EQUIVALENT (MID-RADIUS) BAR INCHES
h = 0·5
h = 0·8
h = 1·2
D = 2·95 · J
POINTS FROM 1300°F COOLING RATE MATCH

FIG. C—Distance along Jominy bar plotted against diameter of equivalent (mid-radius) bar (*see* E. H. Bucknall's contribution)

of fact to any interpretation introducing some criterion of hardness and speak of hardenability, we are liable to fall from grace. The fall will be particularly rapid if the steels being compared differ appreciably in carbon content, in which case a uniform criterion of hardenability will be difficult to name in terms of hardness.

It is, of course, certain that, despite liability to error in any interpretation which goes beyond the point mentioned, steel users will be tempted to interpret further. It is therefore reassuring to find that the Sub-Committee was able to conclude that, provided that due notice was taken of variations of hardenability across a section, a good correlation could be achieved between end-quench results and cross-sectional hardness results on quenched bars. That conclusion is, however, at variance with a recent paper by Manning, of the Battelle Memorial Institute, who asserts a lack of correlation even when the Jominy tests represent points approximately chosen to represent the section of the bar. Manning attributes this to pressure effects in oil-quenching, which I feel to be not a very probable explanation, and I should like to hear Mr. Russell's views on the veracity of this view and of the possibility that lack of correlation is due to variations in the quenching intensity of the bath when bars of varying sizes are being quenched—an effect which could well be associated with inherent differences in the degree of stirring of the oil when different surface areas are concerned.

When trying to interpret Jominy results beyond the safe point, a good deal of encouragement will be found from Mr. Russell's nomograph, but it will leave the difficulty of having to decide upon an approximate value of H for quenching conditions when prediction is being made from a Jominy test to a result on a quenched bar.

Figure B has been replotted from Mr. Russell's nomograph, and shows distance along a Jominy bar against diameter of bar with equivalent cooling rate at the centre, using various assumed values of H from 0·5 to 1·2, the interesting range for oil-quenching. Added to the diagram are points taken from the *S.A.E. Handbook* for the cooling-rate match at 1300° F. The agreement between those points and an imaginary curve for $H = 1{\cdot}0$ is much closer than Mr. Russell's theoretical considerations might have led one to expect. Another rather surprising point shown by this diagram is that, within the range of useful interpretation of the Jominy test, to all intents and purposes 2·7 × (distance along the Jominy bar) can be taken as the diameter of the equivalent oil-quenched round having a centre hardness matching up with the surface hardness of the Jominy bar.

Figure C shows a similar plotting for the mid-radius position of a bar. Very much the same is true as with Fig. B as regards the match with the S.A.E. points for 1300° F. cooling rate, and it is again true that within the range of reasonable interpretation a simple linear relationship applies between distance along the Jominy bar and diameter of an equivalent round, the factor in this case being 2·95.

Discussion on

THE INFLUENCE OF HARDENABILITY ON MECHANICAL PROPERTIES

Mr. **W. E. Bardgett** (The United Steel Companies, Ltd., Stocksbridge, near Sheffield) introduced the discussion, based on Conclusion (11), page 415 of the Symposium, and summarized the relevant sections of the report.

DISCUSSION

Mr. **H. Bull** (Messrs. Brown, Bayley's Steel Works, Ltd., Sheffield) : It is not easy to read this excellent work or to take part in the discussion without thinking back, as a member of the Technical Advisory Committee, to the time when the Hardenability Sub-Committee was suggested and began its work under Mr. Oliver. At that time, as the late Dr. Swinden said in his preface, two questions arose : One was the possible good effect of boron, and the other was the hardenability test to measure the effect. The two things went together, because boron was said to increase the hardenability to such an extent that it would save a good deal of alloy. The prime task of the Technical Advisory Committee was to save alloy, and it was their business to approach these matters in a practical way, so that any test such as the Jominy hardenability test, which would be a ready means of determining hardenability or settling the mechanical properties of a cast, was a good thing to consider.

It is a long time since the Hardenability Sub-Committee was formed. It has done a great deal of work, and in this section it does not seem to have got far away from the practical nature of the work which it was set to do, *i.e.*, to see what use the Jominy hardenability test was in assessing the mechanical properties of various casts of steel, irrespective of the alloy content.

The Jominy hardenability test, to be of any practical value in a steelworks, must afford a quick and ready means of assessing the properties of a cast of steel. I do not think that the work in this section of the report shows that the Jominy hardenability test fails so much as a hardenability test, but it does fail to assess some of the properties which we call mechanical properties, and in particular it fails to give us any measure of the toughness of a cast.

If we accept Conclusion (3) on page 199, " Satisfactory mechanical properties may be obtained with partial hardening ; without, however, carrying out mechanical tests, the actual size which will give the desired properties cannot be determined," we are left where we were, in that if we want to know what the mechanical properties are going to be on any size of bar from any cast, we must take that size of bar, heat-treat it, and make tests ; the Jominy test does not help us. That may be because we in Great Britain lay great emphasis on the need for tough steels.

The next point which comes out of my reading of these papers is that it is suggested that one can exchange hardenability for toughness. I think that the authors are rather shy of drawing that conclusion, and I am not sure that it is rightly drawn. Looking at Section VIᴅ, and particularly pages 280 and 282, there is reference to a very good cast which does not obey the rule ; indeed it is clearly stated that those casts which had the best hardenability had not the best toughness when fully tempered. In particular, it shows that they are the more brittle when they are tempered to higher tensile strengths.

My third point, and what I think is outstanding about the discussions and conclusions in these papers, is the reference many times to inherent differences or properties which still need to be explained. For instance, in Section Vᴀ, page 79, it is clearly stated that " these results appear to indicate the presence of inherent cast variables, distinct from hardenability, which affect impact results." I do not suppose that any alloy steelmaker would doubt that these exist. There are certainly differences in steels made by different processes, and also differences from cast to cast even when the casts are " out of the same stable."

It seems to me that while the Sub-Committee has reached the conclusion that the Jominy test cannot be used to indicate what mechanical properties we are to get from steel, and be a substitute for the tensile and impact tests which are put into our specifications, they cannot leave the matter there, and for that reason Mr. Oliver's statement at the end of the Symposium that further work is to be done is welcomed.

There is no doubt that the steelmaking industry in particular would welcome any ready means of assessing the properties of steel, and assessing them quickly, so that the provision of a substitute for the Jominy test would be useful. It is most desirable that such a test should be capable of application in much the same way that a sample-passer or shift chemist applies his methods when the steel is in the furnace.

There are tests of proved usefulness at which, in their future work, the Sub-Committee might care to look. For example, it has been suggested by Mr. Harry Brearley, in particular, in recent

issues of *The British Steelmaker*, that if a small ingot, say, 4½-in. square and weighing about 1 cwt., is cast at the time that the larger ingots are cast, it can readily be rolled to a bar 4½ in. wide and ½ in. thick, and strips roughly ½ in. square cut crosswise from it will, after hardening and tempering, yield valuable information.

Such a test, when applied to steel intended to be used for pressure vessels or the like, which are to be stressed circumferentially, provides results related to the properties of the pressure vessel, etc., into which the larger ingots are subsequently made. The test gets away from the hardenability question, since it can hardly apply on such a small section, and one has the liberty of hardening the piece in water or oil or air with some expedition. The impact results so obtained are strongly confirmative of the cast-to-cast variation mentioned earlier.

Another test much liked by certain gearmakers is to take the 3-in. or 4-in. round or square bar and cut out and make transverse tests on discs after hardening and tempering. This is certainly of value in that it shows that one cast, tested transversely at a tensile stress of 55 tons, may have an impact value of 5 ft. lb., and another an impact value of 25 ft. lb.

There is also the suggestion, I think in Section VIᴅ, of comparing oil-hardened and tempered pieces with similar pieces which have been partially transformed at 400–450° C. It may be a means of assessing the differences between casts before they are put into production ; I do not know ; but I suggest—and I should like to hear the views of Mr. Oliver or Mr. Bardgett about this—that it would not work particularly well for the alloy steels, because they transform at lower temperatures or transform less readily than would be necessary to show the differences.

The suggestions which I have made up to now have mainly consisted of ways of assessing the goodness of steel by means of a cross test, and usually a cross impact. The papers in this Symposium are singularly free from any such tests, and if it were possible that the information could be obtained on the same large bars as are reported upon here, I think it might be valuable.

There is another omission in these papers. It is not clear sometimes whether the test-pieces were cooled in air or in water after tempering, and I wonder whether temper-brittleness has affected some of the casts, because freedom from temper-brittleness cannot be assured altogether by putting molybdenum into the steel, and it may be that some of the casts were more temper-brittle than others. I do not know whether it is within the province of the Hardenability Sub-Committee to deal with that question, but it is one of the things about which we have no adequate explanation. We are also aware of a trough in the impact curves when alloy steels are tempered round about 300° to 400° C. I think that the toughest steels have the least semblance of a trough. If we can persuade the Hardenability Sub-Committee to pursue their work and tell us a good deal more about the things which must grow out of their present work, it would add to our knowledge of such matters as I have mentioned.

Mr. **F. E. Stokeld** (Birmingham) : This is a subject which is of much interest, and I should like to express my appreciation to the members of the Hardenability Sub-Committee and to all those who have contributed papers, for the large amount of work which they have done and for the admirable way in which the results have been presented. It seems to me that the end-quench test is a simple one, designed to give certain information quickly, and the Sub-Committee has thoroughly explored its possibilities. That is what they set out to do.

As one working in the drop-forging industry, I should like to say that we are particularly interested in the relationship between what we, like many other people, call " hardenability " and the mechanical properties of the different steels which we use. We deal with a very large variety of heat-treatable steels, all of which must give the specified mechanical properties when in the form of forgings.

With some steels our difficulties are few and far between, but with others they are much more evident. My own experience points to two common steels as being the most serious offenders, and the difficulties seem to be due to lack of hardenability in both cases. This phenomenon is the more troublesome to drop forgers because we must endeavour to avoid the use of water-quenching. When water is the quenching medium employed, the hardening operation becomes dangerous owing to the cracking propensities of forgings having unequal sections. However, we do water-quench forgings when it must be done and we find that one of the essential precautions is to ensure that the time between quenching for hardening and charging into the tempering furnace is very short indeed.

Because of the necessity to avoid water-quenching whenever possible, we cannot afford to make forgings from steels possessing abnormally low hardenability characteristics. An instance of a recent occurrence at the works of some of our friends may be of interest in this connection. The normal practice was for a steel of 0·35 to 0·45% of carbon and 0·7 to 0·9% of manganese to be hardened and tempered to give Brinell hardness figures of 229–269. A normal steel will give these figures after oil-quenching and then tempering at

about 500° C. We have had some steels which will give them only after water-quenching, but when the steel is fairly normal, we can temper at 600° C. and still obtain those figures satisfactorily.

We had one particular consignment of steel, however, which was not tested before use. The forgings had a ruling section of about 1 in. and contained pads about $\frac{3}{8}$ in. thick. They were produced from this particular lot of steel and when water-quenched from 850° C. or even higher, they gave only 460–520 V.P.N., whereas a hardness figure of at least 700 V.P.N. is obtained on normal material after quenching only. The McQuaid-Ehn grain-size for both of these steels was much the same (about 6–7) and we considered that this could have had little influence on the matter. Samples of this steel and of a normal steel were submitted to the Jominy end-quench test and the results were compared; very similar curves were obtained and it was apparent that the Jominy test did not differentiate between these two materials.

The other troublesome variety was the carbon–manganese–molybdenum steel usually known as En 16. In this case we were not concerned with surface hardness only, but also with particular difficulties due to the insufficient depth of hardness produced by heat-treatment. This phenomenon first came to my notice early in the war, when supplies of En 16 steel were received from the U.S.A. and we had to make forgings from it to comply with the mechanical-test requirements of the familiar Air Board specification *S*11, which requires at least 55 tons/sq. in. tensile strength.

Up to this time, we had become accustomed to obtaining a range of 255–286 Brinell hardness numbers on batches of forgings in the nickel–chromium (*S*11) steel proper, and test samples from such batches regularly gave an ultimate-stress value well within the lower portion of the specified range, *i.e.*, between 55 and 60 tons/sq. in., without any difficulty. The machinists appreciated this and we had almost no reports of machining difficulties.

When we came to heat-treat forgings made from the substitute En 16 material, however, we ran into difficulties almost at once. Test samples giving normal Brinell hardness numbers failed to give the minimum 0·1% proof-stress and ultimate-stress values required by specification *S*11. These failures were particularly noticeable on samples of ruling sections of $1\frac{1}{2}$–$2\frac{1}{2}$ in., but were also observed down to $1\frac{1}{8}$-in. ruling section. This trouble was not common to all casts of the En 16 material, but at one time it was very prevalent indeed. Casts of En 16 steel made by steelmakers in this country have not been so troublesome, but even now, on British steel, we occasionally have something like this trouble.

To cut a long story short, we made some investigations. As is well known, B.S. specification *S*11 requires the 0·1% proof stress and the ultimate stress to be not less than 43 and 55 tons/sq. in. respectively. We found that in order to ensure that these properties were achieved on axial test-pieces, we had to obtain not less than 269 Brinell hardness numbers on forgings of up to 1-in. ruling section and 286–293 Brinell hardness numbers on thicker ones made from the En 16 that exhibited what we called "low hardenability." Unfortunately, in several instances it became necessary to resort to water-quenching when hardening forgings made from such material and, despite every care, a number of them were cracked and therefore scrapped. This did not help our efforts to achieve a still higher output of good, sound aircraft forgings, and the machinist was also in difficulty because of the greater surface hardness of the forgings which had not cracked. Some attempts were made to use the Jominy test to distinguish one cast from another, but the results were not considered satisfactory and eventually that procedure was abandoned.

We then resorted to another type of test and this has given more satisfaction. It is along the lines already mentioned by Mr. Bull. We hardened and tempered samples from each delivery of bars and billets. After forging samples of the billet to the largest ruling section of the forgings that we intended making from each size and cast, we hardened them in oil and tempered at a standard temperature of 540° C. We then cut through the sample and obtained Brinell hardness numbers on the surface and on the axis of the section. We also made a nicked fracture. From the results of these tests we were able to judge the suitability of the material for the purpose intended. We could also decide whether to use it for that particular purpose, or to set it aside for making other forgings of a smaller ruling section at some future date. In some instances that "future date" is still a date in the future, because there is a remnant of such material still in stock at our works. This material is now being used to make automobile and other forgings to specifications En 16*R* or *S*, which require lower tensile figures than does *S*11.

We have found our method of testing steels for their response to heat-treatment to be of much value and it has brought to light a number of instances where, at the lowest estimate, great difficulties in the final heat-treatment of forgings have been avoided. The method is still in use today on most of the carbon and low-alloy steels that we receive for making forgings which are required to be in the hardened and tempered condition.

Recent instances can be quoted of casts of

carbon–manganese–molybdenum steels of similar composition, grain-size, and end-quench test results, the response of which to our method of testing showed definite differences between them. As an example, let us take two casts, *A* and *B*, for which the following figures were obtained :

	Cast *A*	Cast *B*
Carbon, %	0·38	0·36
Manganese, %	1·6	1·6
Molybdenum, %	0·25	0·27
Surface B.H.N.	286	269
Centre B.H.N.	248	269
Ultimate stress, tons/sq. in.	54·2	57·6

I have occupied the time of the meeting in this way because I wanted to indicate one method by which the matter of variable hardenability was being tackled in actual works practice, and to show the real need which exists for a quick and accurate method of obtaining the desired information before steel is taken into use for processing, prior to final heat-treatment. In the absence of such information serious difficulties can arise which cause loss of output, if not of the product itself. It is part of the duty of the works metallurgist to prevent such avoidable losses. Any improvements in his methods and practices, or any new tools which are available to him for the assessment of material quality, will therefore be most welcome. It is, perhaps, disappointing that the conclusions of the Sub-Committee do not indicate that the end-quench test itself is sufficient for such a purpose.

We have been warned that caution is needed when the end-quench test is considered as a possible specification requirement. I support this very fully, because I fail to see, as yet, how any advantage can be obtained by its use in the field of standard specifications. I feel, however, that much publicity should be given to the finding of the Sub-Committee that a cast of steel cannot be adequately represented by a single end-quench test ; also to the finding that full hardening is necessary to secure optimum mechanical properties. These are important points which need to be kept in mind when future work is undertaken for the improvement of steel quality.

Nevertheless, I feel that, to date, the best test to discover whether a particular batch of wrought steel is capable of acquiring the specified mechanical properties through heat-treatment or not, is to follow the procedure laid down in the appropriate British Standard specification for aircraft steels. Furthermore, I feel that the method used at our works still seems to be the only fairly reliable short cut available as yet, but this, too, has its limitations.

I would like to conclude, as I began, by offering my congratulations and thanks to the Sub-Committee for their able accomplishment and presentation of all the work embodied in this Symposium.

Mr. **E. H. Bucknall** (The Mond Nickel Co., Ltd., Birmingham) : Mr. Stokeld has just been emphasizing the virtues of a " try it and see " policy as regards the use of steels. It seems to me, however, that in setting out on a " try it and see " policy of hardening and tempering a wide variety of steels to decide their suitability for use in particular sections, a great deal of work will be involved, but by using the Jominy test as a preliminary selection test, to eliminate from consideration those materials which are clearly out of court, the task will be greatly reduced.

Modern specifications refer to maximum ruling sections of 6, 4, $2\frac{1}{2}$, $1\frac{1}{8}$, and $\frac{7}{8}$ in., of which the two extremes fall outside the range for which the Jominy test can usefully be employed. For $2\frac{1}{2}$- and $1\frac{1}{8}$-in. dia. sections the specifications demand certain mechanical properties at the centre, while for a 4-in. section mid-radius properties are specified.

On the basis of a conversion method referred to earlier in the discussion, it may be concluded that for a steel to stand any chance of giving the desired properties in $1\frac{1}{8}$-, $2\frac{1}{2}$-, and 4-in. sections it must have an adequate hardness $\frac{7}{16}$, $\frac{15}{16}$, or $\frac{23}{16}$ in. along the Jominy bar. We can go further in eliminating unsuitable steels if for the application in mind it is essential to have a certain margin of hardness as a tempering allowance, even if this is as low as 5 Rc, and in this way reduce the amount of work in testing the suitability of steels very considerably.

A warning to those proposing to take the interpretation of the Jominy test as regards mechanical properties any further is given by Section IVc of the report, where Pumphrey and Jones have described their study of the transformation ranges of hypereutectoid steels by means of interrupted Jominy end-quenches. It is, I think, impossible to exaggerate the value of work in the field between the Jominy test and transformation measurements, as regards finding out the fundamentals of the mechanical properties of alloy steels. The Jominy curve as normally determined gives almost no information from that point of view, although it is possible to infer the *cooling rates* which cause the stage of transformation corresponding to a particular hardness fall on the Jominy curve. Examination of the Jominy curve itself tells one nothing as regards the *temperatures* at which transformation occurred at the important points along the Jominy bar. Microscopical examination of a Jominy bar after the hardness determinations may give vague indications as to the temperature ranges of transformation above the martensite temperature, but without the sort of work described by

Pumphrey and Jones precise information cannot be secured. There remains the possibility of two steels presenting identical Jominy curves and yet one having transformation habits which differ sufficiently for those of the other as to be more dangerous as regards the impairment of mechanical properties, because of a different temperature range of ready transformation.

In a given steel, there is a tie-up between initial hardening and tempering response. This is to quite a large extent covered by contributions in the Symposium, but I think Fig. D crystallizes the

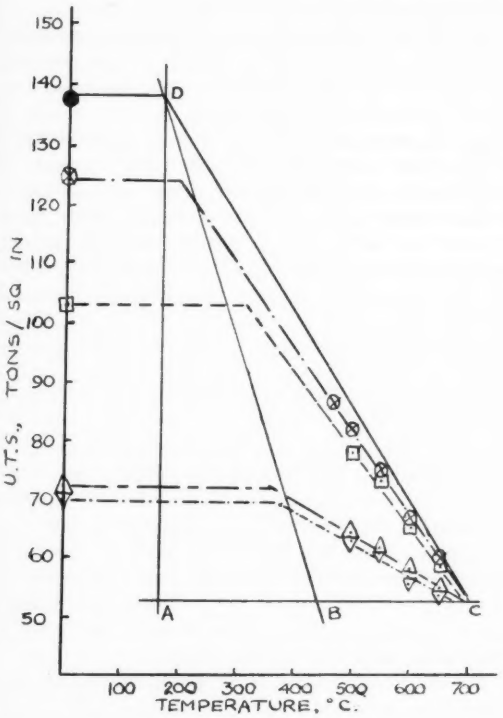


FIG. D—Ultimate tensile strength plotted against tempering temperature for an En 16 steel (*see* E. H. Bucknall's contribution)

matter. The information given in it is particularly pertinent, I think, to this occasion because it is based on data given in the T.A.C. Book B. S. 971 for an En 16 steel. The vertical scale gives tons per square inch, and tempering temperatures are given along the horizontal scale. Ideally this steel should be capable of reaching 138 tons/sq. in. tensile strength, and would then temper as shown by curve *DC*. If the steel is hardened to 125 tons/sq. in. the tempering is as shown in the next curve, and so on for lower degrees of hardening until if it is hardened to only 70 tons/sq. in., the tempering curve is the lowest shown.

Alloy steels differ in the relative position of the curves on such a diagram, and continue to differ in this respect even when the diagram is replotted using degree of initial hardening as the starting point.

On a diagram of this kind impact results can be superimposed as contours, and different steels vary in particular in the way in which these contours run across the hardness curves, so that in different steels, irrespective of their Jominy hardenability, the combination of properties securable after tempering may be distinctly at variance.

This question is being thoroughly examined in The Mond Nickel Company's laboratories at present, and I think it can be said that little support is given by the work to date to the contention once advanced in America that so long as the same microstructure is achieved in quenching, the mechanical properties of all steels are the same after similar tempering. Dr. Swinden in his preface drew attention to a statement by Gillett which took a lot away from that point of view, and it will be a very good thing when it disappears completely, since it is one of those things which, although observed in a particular run of steels, must have been based on a happy chance. For that reason I think that it falls into the same class as the American idea that steels always give a smooth **U**-curve as the hardness traverse across a section.

Lieut.-Col. **T. B. Biernacki** (Polish Technical Institute, London): It seems to me that we expect too much in attempting to predict the mechanical properties of a given steel of a given section from the Jominy hardenability test. This test is obviously limited to only one property, namely, the hardness of a given steel. As is very well known, up to now no relationship has been well established between the mechanical properties and the hardness, unless it is a very approximate one between the tensile strength and the hardness. I am not surprised, therefore, that in this work the attempt to establish a relationship between the results of Jominy tests and the mechanical properties was not successful.

In my opinion, the value of the Jominy test is that we can determine by a very simple method what hardness to expect in a given section of a round bar, and from it we can judge what heat-treatment is required for a given steel. The Jominy test, owing to its simplicity and reproductibility, is a very good tool indeed for all investigations concerning heat-treatment of steel, and particularly the shallow-hardening steels. This is the field in which application of the Jominy test can solve many problems, and where it might show very interesting theoretical and practical results. As regards mechanical properties, we all know that the best mechanical

properties are to be obtained from steel which is fully hardened and then tempered.

Mr. **J. D. Hannah** (Renold and Coventry Chain Co., Ltd., Didsbury, Manchester) : There is some indication that there are fields in which the Jominy test does not fit completely. There is one field, however, in which I think that it does fit completely, but it has not been dealt with by the Sub-Committee, possibly because it gave little chance for the conservation of alloys. It relates to that class of steel mentioned by Dr. Genders in the discussion, *i.e.*, steels used for tools, and in particular for tools used for the production by pressing operations of small parts used in light engineering.

There are innumerable kinds of parts of that description, but the production of certain small-arm-ammunition components—cartridges, bullet envelopes, and so on—may perhaps be taken as typical.

It was in that particular case that I came across some trouble during the war. Certain tools are made for that purpose which consist essentially of a die having a cavity which may be of various shapes, more or less cylindrical or conico-cylindrical, which is hardened by projecting a jet of water into the cavity. It is essential for the usefulness of the tool that the hardened zone shall be restricted and shall fall within certain dimensional limits. If a steel is used which hardens throughout, the tool may be relied on to fail at the first stroke. The steels most used for this purpose contain 1·5–2·0% of carbon and 3–6% of tungsten. The surface hardness of the quenched bore will be 900 Vickers, and possibly more, but the hardness of the relatively unhardened zone will be of the order of 400. The central hardened zone, being permanently expanded as a result of hardening, is in effect inversely shrunk into a surrounding jacket, and it is the system of stresses thereby set up which ensures a compressive stress at the surface of the hard bore serving to balance the bursting tension introduced during the operation of the tools.

These tools are used completely untempered, and it will be seen that the method of hardening presents a close analogy to that which obtains in the Jominy test-piece. Obtaining steels for the purpose of making dies of this sort from perhaps half a dozen steelmakers, all making to the same nominal analysis, and keeping very closely to the same nominal analysis, presented a situation in which the hardenability, represented by the depth of hardening on a particular tool, varied from perhaps 0·03 to 0·04 in. up to as much as 0·375 in. The result, of course, was simply chaos. All that we could do was to adopt a test by making a simple drill hole through a block and using those bars which fell within limits which experience showed to be suitable for the particular job.

It seems to me that the Jominy test would be directly applicable to the exploration of whatever may be the ultimate explanation of such remarkable differences in hardenability and to the specification of steels for tools such as have been described.

Mr. **A. Preece** (Leeds University) : I should like to ask whether the Sub-Committee are contemplating investigations in which the diffusion rates of various elements in the steels will be studied. Fundamentally, hardenability is a problem in diffusion, and work on these lines would appear to be called for.

Mr. **D. A. Oliver**, in his concluding remarks, said : You will appreciate that it is not possible to deal in a wholly adequate way with all the points which have been raised. First of all, I think we must confess as a Sub-Committee that we are wiser after the event than before, and, as Mr. Russell emphasized, there are certain parts of the work which would have been done a little quicker or a little more elegantly had our present experience been available at the beginning of the job. In point of fact we had very little background at that time, and that was our reason for going slowly and attempting to examine all the claims made for the test, even those which from *a priori* reasoning might have been expected to be rather excessive.

I should, however, like to emphasize that quite apart from this question of the Jominy test there is in this Symposium a mass of very careful experimental work on British En steels which can be looked upon as almost an encyclopaedia of heat-treatment. Whether the calculated curves fit the measured curves or not does not detract from the value of the measurements, and I think that point is worthy of emphasis.

Furthermore, I have to announce that the Sub-Committee's life is coming to an end. Its parent Technical Advisory Committee has already passed on with honour, and the Sub-Committee is due to follow it at a not-too-distant date. We are, however, glad that the British Iron and Steel Research Association, through its Thermal Treatment Sub-Committee, will probably absorb a large proportion of the membership, and that hardenability in future will be studied side by side with isothermal and other forms of transformation. I submit that is a complete answer to the large number of speakers who have emphasized the enormous importance of studying hardenability alongside isothermal work, and, as Mr. Preece has rightly emphasized, the diffusion of elements in alloy steels.

The possibility of finding newer tests will be carefully noted. You will gather from the size of the Symposium that we had our work cut out in

adhering faithfully to one test. The work which you have before you was finished in 1945, and the members, having become interested in the subject, have extended it to a far greater degree in certain directions. Possibly the time has now come when new tests can be examined intelligently. The point which Dr. Genders raised this morning as to the value of end-quench testing for the study of tool steels is a particularly good one, and it may well be that this work will find its greatest value in the future in the tool-steel field. For any component which has to be put into use in a condition approximating to the as-quenched state, *i.e.*, the quenched and very lightly tempered condition, a great deal of information can be secured from Jominy tests. Besides tool steels, we have had the example just quoted by Mr. Bardgett of hardened rolls, and another application which comes to mind is that of large, lightly tempered springs. It may prove that in these fields, which were outside the scope of our original terms of reference, there will come a harvest in the future.

We also seem to have discovered a very sensitive test for heterogeneity, and consequently to those interested in exploring ingot structure and uniformity the test may prove useful in that respect.

I shall not occupy your time by repeating those recommendations for future work which have already been printed, but I would say that any new suggestions, other than those raised in this discussion, should be sent to Dr. Becker, at the British Iron and Steel Research Association, and he will transmit them to the reconstructed Committee charged with carrying on this work.

In conclusion, on behalf of the Sub-Committee I should like to thank all those who have obviously spent a great deal of time studying the Symposium, for their interest in it, and for their suggestions for new work which might usefully be carried out. I should also like to add how much the Sub-Committee has appreciated the reception given to this Symposium.

The **President**: We have reached the end of this discussion, and it merely remains for me to thank the authors of the sections of this very valuable Report, the speakers who have presented the Report, and those who have assisted by taking part in the discussion. Mr. Oliver has made clear what was the immediate purpose of the Report. Work had to be done for urgent national reasons, and therefore it is not surprising that attention has been given mainly to the practical application of any experiments made to the production of better steels. I hope that now, as the work is to go on, we shall get down more closely to the fundamental reasons for the behaviour of the different steels. It is on the whole, perhaps, rather disappointing to look through a report of this kind and find how very difficult it has been to obtain any real correlation between the hardenability and either the chemical composition or the microstructure. I hope that it will be possible to carry the work very much further, and particularly, as I am specially interested in the microstructure, to make a very careful study of the kind of changes which go on, making use if possible of the electron microscope, in view of the fact that a great deal of the difference in properties is concerned with the properties of the grain boundaries and not simply of the mass of the grains.

Mr. Service made a very interesting suggestion in speaking of the study of fractures, because the old-fashioned methods of judging steel by fracture, when applied to a specimen which has a range of structure from one end to the other, may give a good deal of information.

The discussion has been very useful, and I can only conclude as I began by saying how very much indebted we are to those who have carried out the immense mass of experimental work and who have put it together in a very clear form, though most of us, I think, find the Report as a whole somewhat tough reading!

The authors' replies to the discussion at the meeting, and the Sub-Committee's considered reply, will be published in a future issue of the *Journal*.

Written Contributions to the Discussion on the SYMPOSIUM ON HARDENABILITY

Dr. **H. O'Neill** and Mr. **J. Dearden** (L.M.S. Railway Company, Derby) wrote, in reference to Section VIIA, by Mr. Glen, that until a few years ago great store was set on the hardenability of steels as an indication of liability to crack during welding. Today it is appreciated that high hardness and proneness to cracking do not always go hand in hand. They had made end-quench tests for instance on two plain carbon spring steels of practically the same chemical composition (including vacuum-fusion results) and hardenability, one of which always cracked during quenching (inherent grain-size No. 5), whilst the other remained sound (inherent grain-size No. 3).

As regards weldability, it was nevertheless helpful to know whether a steel of given composition would give an unduly hard zone in a weld. Dearden and O'Neill[1] had therefore made weld hardenability tests from standardized fillet welds in plates of various *low-alloy* steels of known composition. They obtained the following rough-and-ready relationships, which had received a wide measure of support[2]—perhaps because of their simplicity :

" Carbon hardenability equivalent " (CE)

$$= C\% + \frac{Mo\%}{4} + \frac{Cr\%}{5} + \frac{Mn\%}{6} + \frac{Ni\%}{15}$$

Max. weld hardness, ($\frac{1}{2}$-in. plates, 0·045 sq. in. weld bead) :

$$H_D = 1200\,(CE) - 200$$

The relation between carbon and manganese was subsequently confirmed at the Battelle Memorial Institute, but the value for chromium is in doubt. According to the above expression chromium is reckoned a slightly more potent hardener than manganese in the simplest cases. This view was supported by subsequent published end-quench work of Austin, Van Note, and Prater,[3] but diagrams given by Grossmann[4] and others always show manganese as giving appreciably more hardening than chromium. They had therefore turned to Section VIIA of the Symposium in the hope of obtaining information on this point, but had found it very difficult to unravel the results.

Assuming that all compositions having a 2-in. ideal bar diameter possessed equal hardenability, they had interpolated the following values from Mr. Glen's curves :

Fig. 344 : 0·4% of molybdenum can replace 0·2% of carbon.
Fig. 343 (*a*) : 0·4% of molybdenum can replace 0·6% of manganese.
Fig. 342 : 0·6% of chromium can replace 0·6% of manganese.
Fig. 341 : 1·5% of nickel can replace 0·6% of manganese.
Fig. 360 (*a*) : 0·65% of molybdenum can replace 1·1% of chromium.
Fig. 360 (*a*) : 0·2% of molybdenum can replace 0·1% of carbon.
Fig. 360 (*a*) : 0·35% of chromium can replace 0·1% of carbon.
Fig. 360 (*a*) : 0·4% of manganese can replace 0·6% of chromium.
Fig. 360 (*a*) : 1·0% of nickel can replace 0·6% of manganese.

The values from Fig. 360 (*a*) referred to a 4-in. ideal bar diameter.

Some of these values were contradictory, but in general it could be concluded that carbon was the most potent hardening element, followed by molybdenum, manganese, and chromium, and finally nickel. They were therefore very surprised to find in Fig. 340 that carbon between the values of 0·25% and 0·5% had apparently no effect on the ideal bar diameter, although the latter was doubled for a 0·4% increase in manganese. This result would certainly not apply for hardening by welding.

The alleged decreased multiplying effect with each succeeding increment of alloy element appears to be an arithmetical and not a metallurgical effect. Thus, according to Fig. 341, the slope of the line for the nickel-free steels shows that each 0·1% increment of manganese increases the ideal bar diameter by 0·13 in. independently of the actual manganese content. Surely this straight-line relation indicates constant hardenability effect due to manganese within the range examined! At low manganese contents and small diameters the 0·13-in. increase naturally creates a larger multiplying factor than it does

at high manganese contents and large diameters. The effect of the multiplying factor is arithmetical and nothing more. On this account, conclusions (1) and (2) (page 395) cannot be sustained ; and it is noted that neither of them appear in the Principal Conclusions of the Hardenability Sub-Committee (Section VIII).

This raises the question as to how hardenability should be assessed. Mr. Glen carried out Jominy tests and measured the distance of the point of inflection on the hardness curve from the quenched end of the specimen. He next converted this into ideal bar diameter by means of Grossmann's curve (Fig. 338). The ratio of the ideal diameters of a steel with and without an alloy addition was then taken as a measure of the effectiveness of that addition on the hardenability of steel.

The writers felt that this conception of hardenability was very much removed from reality, and determination of the point of inflection of the curves must be rather difficult. The actual hardness values of the steels concerned did not appear to enter into the calculation, although closely connected with hardenability.

There seemed to be a slip in the last paragraph of page 399 where it is stated that steel *A* has a lower hardenability than steel *B*, whereas the second paragraph and Fig. 360 (*a*) indicate that the steels have the same hardenability and will give a 4-in. ideal diameter.

Another point of interest in Mr. Glen's paper was the hump in the Jominy end-quench curves for chromium steels shown in Fig. 339 on page 366. The separate report would be awaited with interest, but the writers would like to suggest that the tendency for a hump to form is present in many materials. Thus on page 104 the silico-manganese steel 65 shows the same effect, whilst the silico-manganese steel in Fig. 22 shows a significant recalescence on the time–temperature curves. The industrial production of Sandberg sorbitized rails resembles in some ways the end-quench test, and the writers had frequently found a slight hump when exploring the hardness gradient from the water-cooled running surface into the centre of the rail head. A similar effect has been noticed in an L.M.S. procedure devised for the hardening of rail ends, and the presence of a kink in the hardness curve running from the plane of welding of flash-butt-welded rails suggested the same sort of transformation effect. It is hoped to record details of these features elsewhere.

In studying the hardenability of low-alloy steels for welding and of plates for locomotive laminated springs, it is convenient to use a thinner end-quench test-piece as it can be machined from actual deliveries of the steel concerned. A number of tests with a specimen $\frac{7}{16}$ in. in dia., and $1\frac{1}{2}$ in. long have been made, though complete comparisons with the standard 1-in. bar are not available. A conversion curve has been given by Stout, Tor, and Doan,[5] and for a distance of 0·5 in. on the standard bar the equivalent distance is about 0·53 in. on the $\frac{1}{2}$-in. bar. In some of this work comparisons were made between a steel containing 0·24% of carbon and 1·8% of cobalt, and another with 0·25% of carbon and 1·9% of nickel. The natural hardness in each case was H_D 190, but end-quench tests made in 1940 showed that the cobalt steel gave a much shallower depth of hardening than the nickel steel or a plain carbon steel containing 0·26% of carbon. These tests confirm the views of Kramer, Hafner, and Tollman[6] that cobalt decreases hardenability.

REFERENCES

1. J. Dearden and H. O'Neill : *Transactions of the Institute of Welding*, 1940, vol. 3, No. 4, pp. 203–214.
2. H. W. G. Hignett : *Ibid.*, 1943, vol. 6, No. 3, p. 99.
3. C. R. Austin and others : *Transactions of the American Society for Metals*, 1943, vol. 31, Sept., p. 517.
4. M. A. Grossmann : *Metals Technology*, 1942, June, Technical Publication No. 1437.
5. R. D. Stout and others : *U.S. Welding Research Supplement*, 1943, Sept., p. 423s.
6. R. Kramer and others : *Metals Technology*, 1943, Sept., Technical Publication No. 1636.

Mr. **H. S. Bavister** (Vauxhall Motors, Ltd., Luton) wrote : In looking through the many papers contributed to this Symposium, one must initially pay a very real tribute to the large mass of accurate and detailed data which has been presented ; it all merits much study and thought and it seems impossible to do this adequately in the short time available since publication.

As a user of the Jominy test to some extent since 1938, I would express the opinion that it is profoundly a useful test—measuring as it does by relatively simple procedure, a fundamental aspect of the utility of any steel, *i.e.*, its hardenability. We have used the test principally in confirming judgments of the core strength of alloy case-hardening steels, and it has measured the rather subtle distinctions of hardenability between specifications En 34, En 35 *A*, and En 35 *B*, with a degree of accuracy beyond that which could ever be interpreted by the bare figures of chemical composition and grain-size. Thus far, I feel there is no question but that the test is supremely practical and useful to any user of hardenable steels. With regard to the general interpretation of the Jominy results in terms of various-sized bars or of actual components, the position appears as yet to be incapable of exact or certain clarification or generalization.

In our applications of the test we have had to make the obvious comparisons, *i.e.*, measure

XUM

the hardness of case-hardening steels at various positions in the finished components when the latter have been made of casts of known Jominy characteristics—thus to determine the Jominy-test-bar position of various critical positions of our actual components, as produced under our standard production conditions. The same simple procedure can, of course, be adopted to determine whether any steel has adequate depth-hardening characteristics to harden through reliably any particular component.

There are, of course, some variabilities of results, but we must recognize that there are serious overriding factors always with us, for example :

(1) Lack of homogeneity throughout the steel cast.
(2) Hardenability of a steel is affected by previous thermal history and structural conditions.
(3) The fact that our production oil-quenching tanks are pieces of equipment of very variable, and often very low, efficiency.

To summarize, it is my opinion that the Jominy test is now a known profoundly useful tool to measure a most important property of steels, and that this excellent Symposium amply illustrates the need for all of us to build up as much experience as possible to expedite the time when we can use this test to increase the efficiency and economy of the steels used for our various heat-treatment purposes.

Lieut.-Col. **T. B. Biernacki** (Polish Technical Institute, London) referring to the influence of composition on hardenability, wrote : Grossmann's method of calculating hardenability from chemical composition is based upon several assumptions, and only further investigations will show the real value of these assumptions and of the method.

Mr. Glen's paper and also some recent American investigations on this subject do show that the influence of alloying elements in steel upon hardenability is much more complex than Grossmann imagined. In particular it seems that Grossmann's assumption that each alloying element present in steel acts independently upon hardenability is open to question, especially in the case of higher-alloyed steels where two, three, or more alloying elements are present. Here, as one of the speakers suggested, Grossmann may be turning the wrong corner.

The criterion of hardenability, expressed as a depth to which a given section of steel can be hardened, and based on a non-existent "ideal critical diameter" containing at the centre 50% of martensite, may be open to criticism. Apart from the difficulty in estimating the 50% of martensite in the steel by micro-examination, it seems that in practice this criterion of depth hardening is of little value. The practical man is no doubt much more interested in a diameter not just "half-hardened" with 50% of martensite in the centre, but fully hardened with 90–100% of martensite in the centre, because this will guarantee the best mechanical properties after hardening and tempering a given section of steel. But despite the above criticism it should be said that Grossmann's method of approach to the exceedingly complex problem of calculating the hardenability from chemical composition is the best at present available.

Some of Grossmann's assumptions upon which his method is built may be wrong, but it must be admitted that in spite of its limitations it has been possible by this method to establish with sufficient accuracy the influence of minor additions of alloying elements upon hardenability.

I entirely agree with Mr. Glen's opinion that it is not only the first attempt, but also the first fully developed method to place hardenability on a truly quantitative basis, and is undoubtedly a big step forward towards increasing our knowledge of the action of alloying elements in steel, on which Grossmann should be congratulated.

Mr. **W. I. Pumphrey** (Birmingham University) and Dr. **F. W. Jones** (Brown-Firth Research Laboratories, Sheffield) referring to the correlation between the Jominy test results and quenched cylindrical bars, wrote : The careful

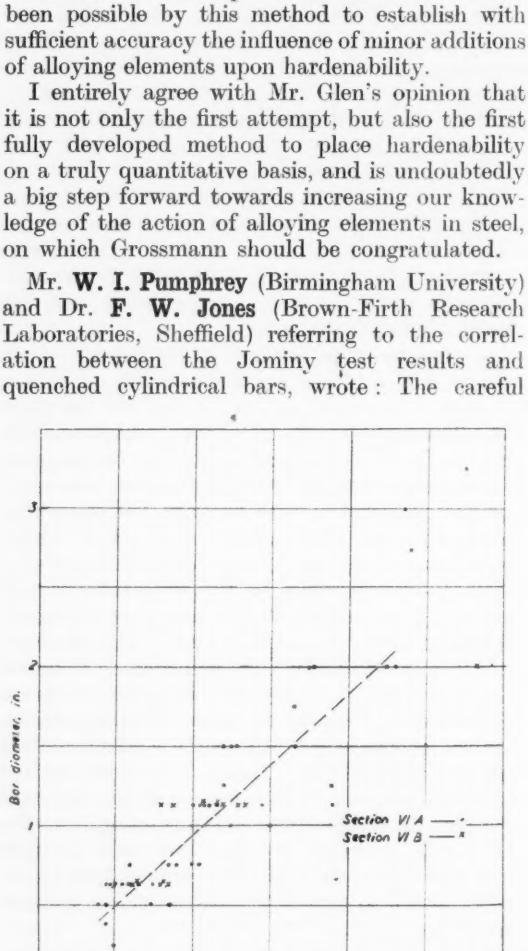


FIG. E—Results abstracted from Section VIA and VIB of the Symposium (*see* W. I. Pumphrey and F. W. Jones' contribution)

work carried out by the Jominy committee described in Section VIA, and by Allsop and Steven in Section VIB, shows that any attempt to predict *accurately* the hardness in quenched cylinders from the Jominy test results will involve considerably more work than the method of " try it and see." Nevertheless there is no doubt that a rough correlation exists between the hardness on the Jominy bar and in quenched rounds ; indeed the Jominy bar test would serve no useful purpose as a measure of hardenability if there were no such correlation.

From the practical point of view the problem is to determine at what distance from the quenched end of the Jominy bar we may expect the same hardness as at the centre of a quenched cylinder of a certain diameter. Figure E shows some results abstracted from Sections VIA and VIB of the Symposium. The centre hardness at the centre of the cylinder was estimated from the hardness traverse curve, and the distance from the quenched end of the Jominy bar having an equivalent hardness was then obtained from the Jominy curve. No results have been used where the hardness was on a relatively flat portion of the Jominy curve.

Figure E shows a large scatter, as might be expected, but the correlation is good enough to be quite useful. For example, if a steel is required which will give a certain hardness in, say, $1\frac{1}{8}$-in. dia. bar, the correlation permits

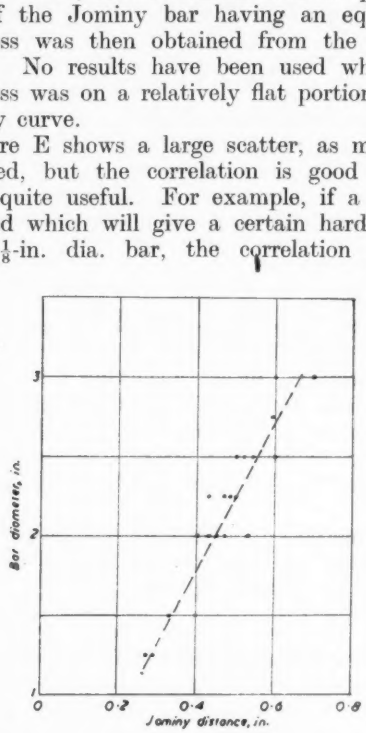


FIG. F—Results similiar to those in Fig. E, abstracted from a paper by Asimow, Craig, and Grossmann (*see* W. I. Pumphrey and F. W. Jones' contribution)

steels to be divided into three classes : (1) Those which are definitely too shallow-hardening, (2) those which are definitely adequate, and (3) a third marginal class for which quenching experiments on actual bars are required before a decision can be made. In this way the amount of work involved in selecting a steel for a particular purpose may be materially reduced.

Figure F shows some similar results for water-quenching abstracted from a paper by Asimow, Craig, and Grossmann.* As with Fig. E the bar diameter is that which gives a hardness *at the centre* equal to that found at the corresponding position on the Jominy bar.

It should be noted that the correlation obtained in this way is purely experimental and does not involve any assumptions regarding the constancy of the heat-transfer factor h and the thermal properties of the steels, or of the choice of the correct transformation range. The validity of these assumptions has been questioned elsewhere in the discussion, so that a method of correlation which avoids them is to be preferred.

In addition, from the method of correlation used in Section VI (which involves the determination of the effective value of h), it is not clear what " scatter " may be expected in the Jominy distance corresponding to the centre of a certain diameter of quenched round. This information is immediately apparent from the results of Figs. E and F, although it should be emphasized that these figures are based on somewhat limited data and may require slight modification as further results become available.

Dr. **F. W. Jones** (Brown-Firth Research Laboratories, Sheffield), referring to Section IVB on surface temperature measurements on Jominy bars, wrote : The authors find an initial drop in temperature of about 30° C. which occurs between the removal of the Jominy bar from the furnace and the start of the quench. It appears unlikely that this fall in temperature is due to air-cooling of the bar. The time from furnace to quenching fixture is of the order of 3 sec. The cooling curves at 2, $1\frac{1}{2}$, and 1 in., will in the early stages be fairly similar to the curve obtained with air-cooling, at any rate they will certainly not be slower. If we extrapolate these curves back to $t = -3$ sec., we find that the drop in temperature due to air-cooling during the time the Jominy bar is transferred to the quenching fixture should not be more than 8° C., which is much smaller than the observed drop of about 30° C. We suggest, therefore, that the larger part of this initial drop is not a true fall in temperature of the bar, but is in fact a thermocouple effect.

The two thermocouple wires were welded individually to the surface of the bar, a procedure which gives good results as the surface of the bar itself becomes a part of the hot junction, and it is therefore certain that the bar temperature and not merely the junction temperature of the couple is being measured. However, although

* *S.A.E. Journal*, 1941, vol. 49, No. 1, p. 283

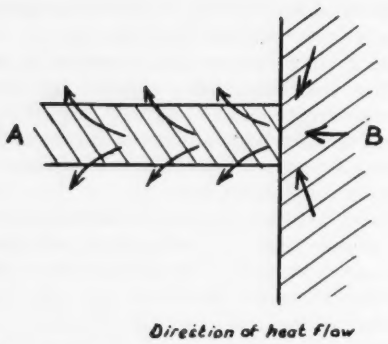


FIG. G—Heat loss from couple wire attached to bar (*see* F. W. Jones' contribution)

this method of attaching the couple would appear to be the best practicable, there is still the possibility that the surface temperature of the bar, at the points of contact of the couple wires, may be lower than at other positions on the surface.

Figure G shows one couple wire A welded on to the bar B. When the Jominy bar is pulled out of the furnace, the thin wire A will cool very much more rapidly than the bar. Heat will therefore be conducted away from the bar along the wire, and we suggest this causes the surface temperature of the bar, at the junction AB, to fall below the temperature of the surface at positions distant from the junction.

The error introduced by conduction along the couple wires may be allowed for, to a first approximation, by the following method :

If steady conditions of heat loss by the couple wire are realized within about 3 sec. of the removal of the bar from the furnace, then during the cooling of the Jominy bar in the quench, the junction AB will be at a lower temperature (T) than the rest of the surface of the bar, at the same distance from the quenched end (temperature T'). To a first approximation the temperature error ($T' - T$) will be proportional to the temperature excess of the bar over that of its surroundings (T_2). That is :

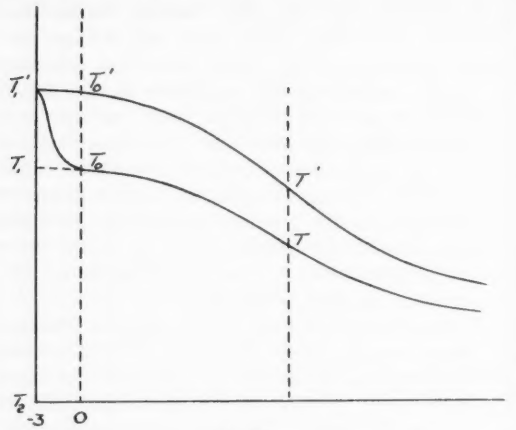


FIG. H—True and observed cooling curves of Jominy bar (hypothetical) (*see* F. W. Jones' contribution)

$$T' - T = c\,(T' - T_2)$$
$$\therefore\quad T' - T_2 - T + T_2 = cT' - cT_2$$
$$\therefore\quad (1 - c)\,(T' - T_2) = T - T_2$$

The fractional temperature :

$$U = \frac{T' - T_2}{T'_1 - T_2} = \frac{T - T_2}{T_1 - T_2},$$

where T'_1 is the furnace temperature, and T_1 is the temperature which would be observed at $t = -3$ sec. if the thermocouple wires were subjected to air-cooling. It may be obtained approximately by extrapolating the time–temperature curve back to the time $t = -3$ sec.

(*See* Fig. H which makes clear the meaning of the the symbols used above.)

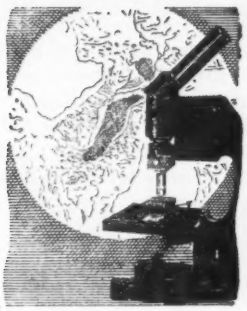
If, therefore, when calculating the fractional temperature U, instead of taking the furnace temperature for T_1, we obtain T_1 by the method suggested above, the errors due to heat loss along the couple wires should be considerably reduced.

This method of calculation will give temperatures (and cooling rates) round about 2–3% higher than Russell's figures.

The replies to the written contributions to the discussion will be published in a future issue of the *Journal*.

B. I. S. R. A.

BRITISH IRON AND STEEL RESEARCH ASSOCIATION



Installation and Use of Instruments on Open-Hearth Melting Furnaces*

By R. C. Baker, A.Met., A.I.M.†

SYNOPSIS

The installation and development of recording and control instruments for routine use in a small open-hearth plant are described. Features of the system are the recording of the furnace-roof temperature and the gas and air flows on one chart, also an experimental automatic crown-temperature control. The instruments have proved of great value to the operators and have increased the melting rates.

I—INTRODUCTION

THE Open-Hearth Instruments Sub-Committee of the British Iron and Steel Research Association has for some time been studying the application of instruments to open-hearth furnaces. The Committee has invited reports on the experience of melting-shop managers with such instruments, and these notes describe the installation and development of instruments for routine use in a small open-hearth plant consisting of one acid Venturi furnace of 50 tons capacity and a basic furnace of 40 tons capacity, making steels used mainly for the manufacture of railway tyres and wheels. The installation of these instruments has been carried out gradually over a number of years as the necessity for further instrumentation became apparent.

Previous to 1930 it was obvious to the management at the works with which the author is connected that the draught in a Siemens open-hearth furnace had an effect on the flame, and a rough draught control was installed; this was operated on the draught at the top of the air checker, and, although it was not very sensitive, it showed that this was a move in the right direction, and the instrument was in use for some years. This control was replaced by a Hopkinson-Carlstedt regulator when it was first introduced into this country, in 1936. Shortly afterwards gas- and air-flow indicators were installed, followed by recorders for these indicators.

The present instruments are as follows:

(1) Automatic furnace-draught control (now usually referred to as furnace-pressure control).

(2) Producer-gas- and air-flow indicators and recorders.

(3) Gas and air checkers waste-gas temperature recorder.

(4) Furnace-roof temperature indicators and recorders.

(5) Schofield-Grace immersion pyrometer for liquid-steel-temperature measurement.

More recently the gas and air flows and the roof temperature have been recorded on one recorder chart instead of on three separate charts.

Our present aim in using these instruments is,

* Paper submitted to the Open-Hearth Instruments Sub-Committee of the Steelmaking Division of the British Iron and Steel Research Association. Received 1st April, 1947.

† Messrs. John Baker and Bessemer, Ltd.

by measurement of some of the variable factors, to give the furnaceman a means of maintaining a high average melting rate and eventually to arrive at some simple instructions for the control of a furnace during the charging and melting period. As a first step towards this, we consider that these instruments give the furnaceman a means of ensuring (*a*) that the furnace-roof temperature is maintained at a previously determined limiting temperature for as long a period as possible until the charge is melted and a sufficiently high bath temperature is reached for refining, (*b*) that a fixed maximum flow of gas is maintained whenever the roof temperature is below this limit, (*c*) that the corresponding flow of air necessary to burn this gas is maintained, and (*d*) that the furnace draught or pressure is controlled.

We are now of the opinion that two further instruments should be installed, namely :

(i) Automatic crown-temperature control.

(ii) Automatic air/gas ratio control, with a method for altering the ratio.

The apparatus for controlling the gas flow from the crown temperature is at present in the experimental stage and will be mentioned in a later section of the paper. The following descriptions of the instruments are given in the order in which the instruments were installed.

II—The Instruments and Their Functions

(1) *Automatic Furnace-Draught Control*

Furnace-draught control is more usually termed furnace-pressure control, due to the point of control being at crown level, but in this case the control point is at a lower level, and the term " furnace-draught control " is more appropriate.

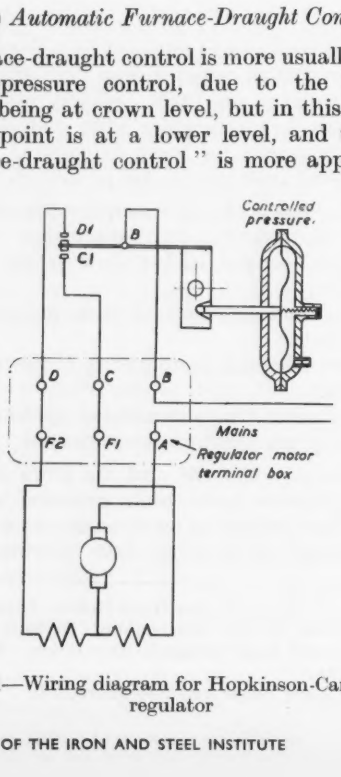


Fig. 1—Wiring diagram for Hopkinson-Carlstedt regulator

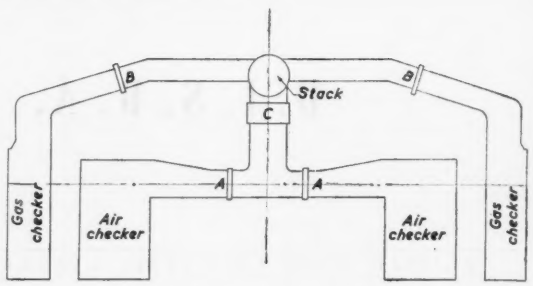


Fig. 2—Layout of gas and air flues

We consider that automatic draught control is essential, as the amount of " cold-air infiltration " is largely dependent on the draught, and for this reason it can completely alter the character of the flame, *i.e.*, with fixed gas and air flow, increasing the draught shortens the flame. In view of this fact we maintain that gas- and air-flow meters are of very little use except in conjunction with furnace-draught control.

(a) *Type of Control and Methods of Use*

As previously mentioned in the introduction to these notes, a Hopkinson-Carlstedt regulator—a diagram of which is shown in Fig. 1—was installed on one of the furnaces in 1936, and arranged to control the waste-heat-boiler damper. The regulator consists of a diaphram connected at one side to the draught to be controlled and balanced by a spring in compression which can be set to any predetermined value representing the desired control value. This diaphram in turn operates a set of contacts which control a reversible motor, the motor having sufficient power to control a light damper.

This regulator is used either (*a*) to control a waste-heat-boiler damper, or (*b*) the control is arranged to allow a regulated amount of cold air to be drawn in at the bottom of the furnace stack

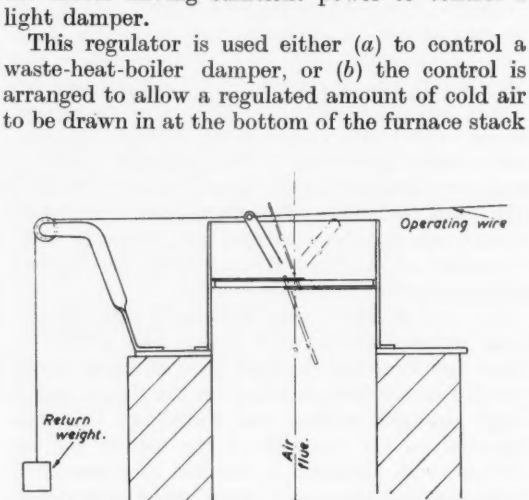


Fig. 3—Pivoted butterfly valve bricked into air flue

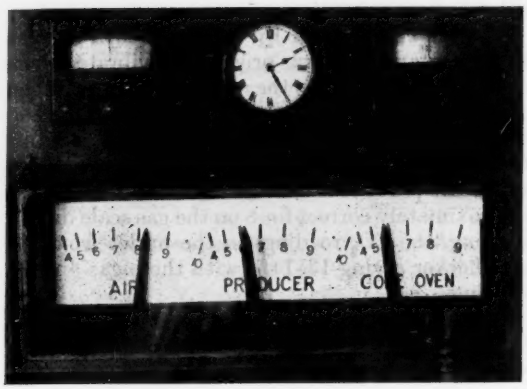


FIG. 4—Air-, producer-gas- and coke-oven-gas flow indicators of an early type

by the method to be described. The layout of gas and air flues on the furnace in which method (*b*) is used is shown in Fig. 2.

The waste gases from the checkers are approximately proportioned by the amount of lift on the air dampers *A* and the gas dampers *B*, which can be preset to any required amount of lift.

Provision is made for air to be drawn in near the bottom of the stack through a pivoted butterfly valve mounted in a cast-iron box bricked into the air flue at position *C*. This is shown in more detail in Fig. 3. The Hopkinson regulator is arranged to control this pivoted butterfly valve, thus allowing a regulated amount of cold air to be drawn into the stack and in this way controlling the draught in the furnace chamber.

(b) *Position of Control Point*

The control was at first operated on the draught at the top of the outgoing air checker, a simple reversing valve operated by the air reversing lever being used to connect the control to the correct checker. This point of control was found to be unsatisfactory, particularly on the acid furnace, due to the air downtakes making-up with slag, and later the control point was moved to a position on the front wall of the furnace, 18 in. above sill level, between the middle and end doors. The control setting was decided by visual inspection of the amount of flame blowing at the top of the furnace doors, and set to give slight blowing at this point. Later, when a micromanometer was available, a control setting at this point of 0·030 in. W.G. draught was adopted as being the most suitable, consistent with the opposing factor of too much flame blowing to make the working of the furnace reasonable. It is now considered that a lower control setting (*i.e.*, increased furnace pressure) would be advantageous and practicable.

We have found that it is essential to work this control at a slow rate of response (*a*) in order to avoid hunting, and (*b*) so that the control does not cause a big increase in draught immediately the furnace doors are opened.

(2) *Producer-Gas- and Air-Flow Indicators and Recorders*

The gas and air flows are indicated by means of ring balances fitted with pointers and are arranged to read from 0 to 10 on a scale 10 in. long, giving very clear indication. An instrument panel containing three of these indicators for air, producer-gas, and coke-oven-gas flow is shown in Fig. 4, and the back view of the same panel, showing the ring gauges and recording charts, is shown in Fig. 5.

(a) *Gas Measurement*

The gas flow is measured across the water seal of the reversing valves on each end of the furnace, owing to there being no suitable length of straight vertical flue in which to put an orifice. A large amount of work[1, 2] has been published on the question of gas measurement, and consequently we intend to mention only the particular difficulties encountered in choosing a suitable point of measurement in our restricted layout of flues.

The main gas culvert from the producers is underground and is of considerable length; on reaching the furnace the culvert is connected through the furnace regulating valve to an overhead flue, and from this to two water-sealed reversing valves of the type shown in Fig. 6. These valves are either closed, as shown in Fig. 6 (*a*) or fully open as in Fig. 6 (*b*). The water seals of these reversing valves are used as the measuring orifices for each end of the furnace. One of the valves is shown in more detail in Fig. 7,

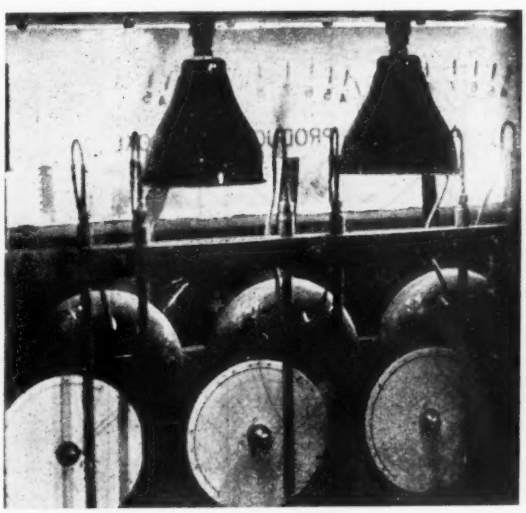


FIG. 5—Back view of instrument panel shown in Fig. 4, showing ring gauges and recording charts

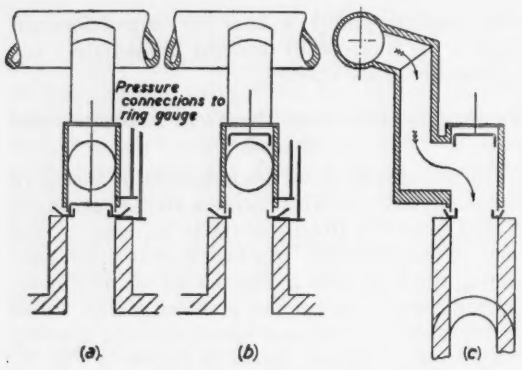


FIG. 6—Water-sealed reversing valves in (*a*) closed and (*b*) fully open position

which also shows the position of the pressure tappings. These pressure tappings from each end of the furnace are connected to the ring gauge through a simple reversing valve worked from the furnace gas reversing lever. The pressure tappings require periodical cleaning, and Fig. 7 also shows the method of cleaning these pipes by occasionally pushing the rods, *A*, through the pipes. We have found that a supply of air to these pipes is not a satisfactory method of keeping them clean.

In order to calibrate the gas recorder for reference purposes a set of charts for a complete week are planimetered and the total number of units divided into the coal consumption during this period ; naturally, this calibration has to be done when only this particular furnace is in operation. The figure 8 on the gas scale corresponds to 204,000 cu. ft. of gas per hour.

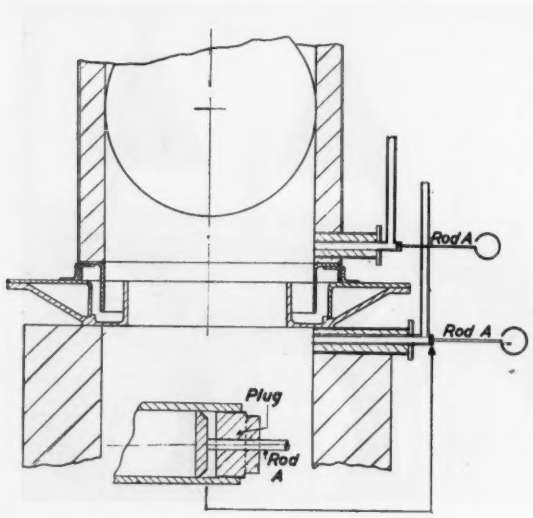


FIG. 7—Detail of water-sealed reversing valve and method of cleaning pressure tappings

(b) *Air Measurement*

The air is measured by an orifice on the inlet to the air fan which was originally calibrated, but, owing to alterations to the orifice to give increased air supply to the furnace, the present calibration is not known. We now intend to build a Venturi on to the fan inlet and so obtain a definite calibration. In practice 8 on the air-indicator scale is approximately correct for 8 on the gas scale during melting, the CO_2 readings in the outlet from the air checker giving 13–14% with these gas and air readings.

Recently these flow measurements have been transferred electrically to a six-point recorder which also records the crown temperatures, and this will be discussed in a later section of the paper.

(3) *Gas and Air Checkers Waste-Gas-Temperature Recorder*

The temperatures in the waste-gas flues from the gas and air checkers are recorded, using an ordinary base-metal thermocouple in a silica sheath, but this information is intended to be a guide only to the melting-shop manager, and the furnaceman is not expected to make use of these records.

The normal range of temperature during a reversal is 100–800° C. on the outgoing air flue, and 500–700° C. on the gas flue.

(4) *Furnace-Roof-Temperature Indicators and Recorders*

The limiting factor which determines whether a furnaceman has to cut down the gas input to his furnace is the crown temperature, and for this reason we decided that it was essential that this temperature should be measured. When this was first attempted there were only Fery radiation pyrometers with a glass screen available in the works ; the early experimental work was thus done with these pyrometers, and later they were adopted as being suitable.

At first a single Fery radiation pyrometer was

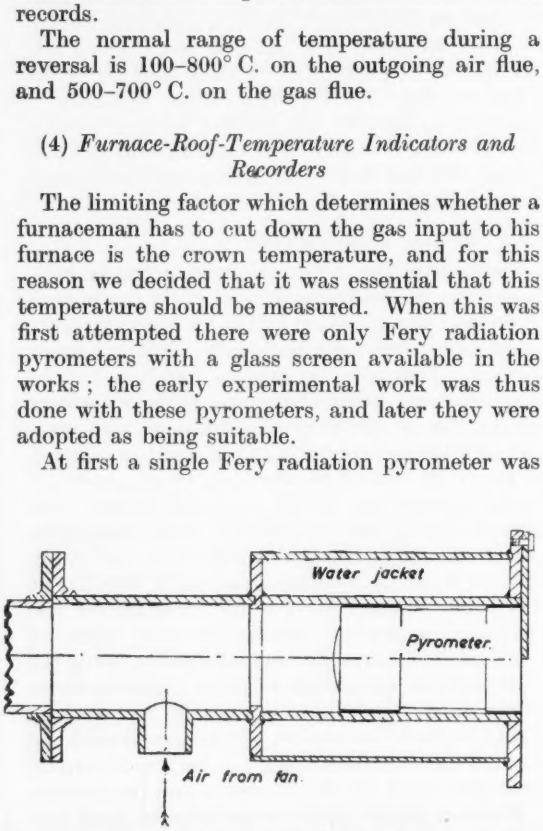


FIG. 8—Roof-pyrometer housing

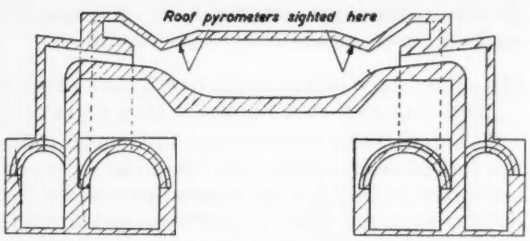


FIG. 9—Sighting positions for roof pyrometers

sighted through a hole in the back wall on to the inside of the crown, no means of protecting the pyrometer being employed; this was naturally not very satisfactory. Later, owing to the valuable development work on the use of radiation pyrometers in suitable protective housings for this purpose, carried out by Mr. F. L. Robertson and his colleagues at Shelton and later at the Shotton works of Messrs. John Summers and Sons, Ltd., we were able to design a similar housing suitable for the Fery radiation pyrometers. We now use the simpler housing shown in Fig. 8, in which fan air drawn through filter bags is used. Two Fery pyrometers are used, as normally the outgoing end of the crown is hotter than the incoming end. The particular sighting position selected (shown in Fig. 9) is a compromise between the hottest portion of the crown and a position where flame interference can be avoided.

In conjunction with these pyrometers we have found that illuminated scale indicators are easily readable, even at a distance, and two of these are now incorporated in each of our furnace instrument panels (*see* Figs. 4 and 13). The temperatures were at first also recorded on two single-point recorders, but, owing to the small size of the charts, these were not satisfactory.

Calibration

These pyrometers are checked once per shift by removing the radiation pyrometer and sighting a disappearing-filament pyrometer through the protection tube, then replacing the radiation pyrometer, and, as soon as the illuminated scale indicator has reached a steady temperature, comparing the readings. This calibration is normally carried out under the following furnace conditions:

(1) When the roof temperature is reasonably constant, *i.e.*, during the refining period.

(2) When the furnace doors are closed.

In reporting the result of the calibration, we

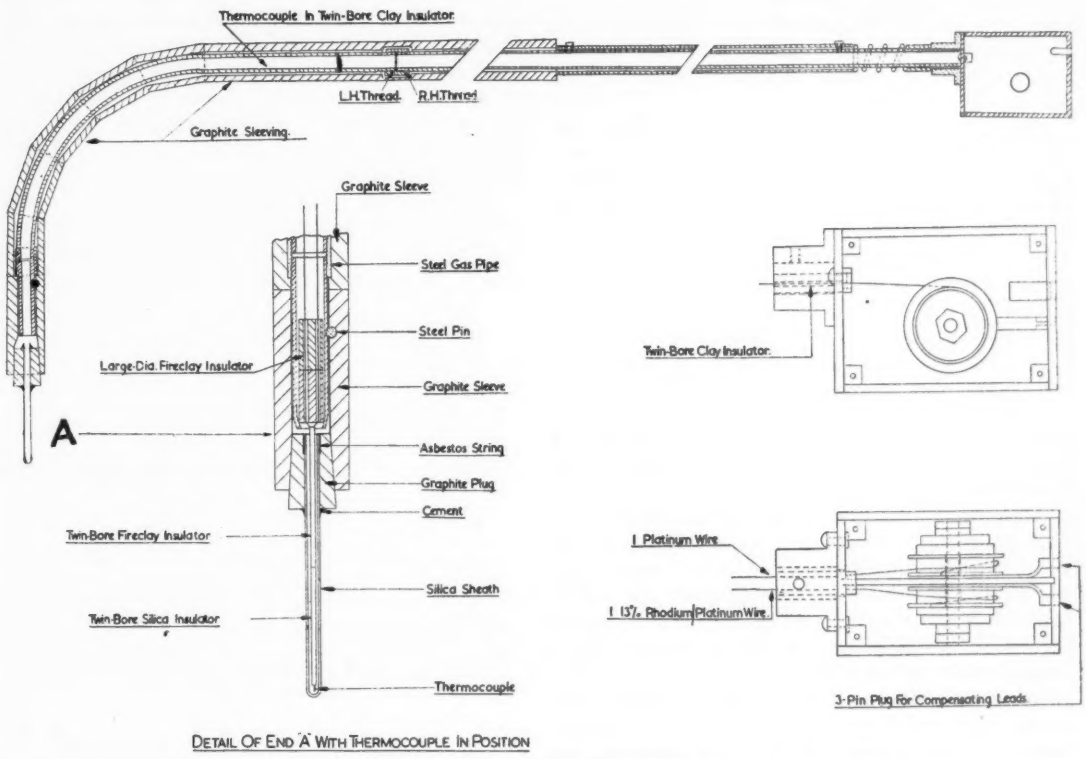


FIG. 10—Diagram of modified immersion pyrometer as suggested by Land

consider that it is not good policy to tell the furnaceman that his instrument is reading a certain amount low, but if the instrument gives an incorrect reading the fault should either be remedied immediately or, if this is not possible, the pyrometer should be removed until an adjustment can be made. It will be remembered that these points were stressed by Dr. Chesters in a recent report,[3] and we entirely confirm his views.

In practice it is found that the readings normally agree, but if there is any appreciable variation it is generally due to distortion of the protective housing at the hot end leading into the furnace. Incorrect readings due to the sight hole being blocked with pieces of brick, etc., are normally obvious to the furnaceman, as they affect the readings drastically ; owing to the design of the housing, the tube can easily be cleaned out. The greatest difficulty is to make the furnaceman realize that these pyrometers will not indicate the temperature of the whole crown ; it is therefore possible, due to the ports being of the incorrect size or shape, to overheat local parts of the crown under exceptional conditions, but we see no way out of this difficulty.

(5) *Schofield-Grace Immersion Pyrometer for Liquid-Steel-Temperature Measurement*

The Schofield - Grace immersion - pyrometer method of measuring liquid-steel temperatures has now been in use for several years, and as it is a routine measurement in many melting shops no attempt will be made to describe the method in detail.

Owing to the restricted size of the shop the standard type of pyrometer mounted on a trolley is not suitable, and a modification of this design, which can easily be carried, is used in conjunction with a Tinsley amplifier and high-speed recorder. Very few troubles have been encountered with this equipment until recently, when we have had difficulties due to moisture on the compensating leads and fireclay insulators inside the pyrometer tube. For this reason we are now using a modified pyrometer suggested by Land[4] which we consider may overcome this difficulty and at the same time require less maintenance than the previous type of pyrometer. A diagram of this pyrometer is given in Fig. 10.

A general instruction is issued to the furnacemen that they may use these pyrometers whenever they consider it necessary, and their frequent use has proved that they appreciate that pyrometers are now a necessity. In addition, for purposes of record, a temperature reading is always taken immediately before tapping.

A considerable number of these pyrometers are available in order that (*a*) all maintenance work, including the renewal of the silica sheaths, can be carried out during the day shift, and that (*b*) the melter need not fall back on empirical methods, even under exceptional conditions.

III—The Recording of Crown Temperature and Gas and Air Flow on One Chart

To obtain the best results from instrumentation, the provision of suitable recording charts, easily interpretable both by the furnacemen and by the melting-shop manager, is essential, and for this reason we decided to record the crown temperature and the gas and air flow on one chart, as previously we had considerable difficulty in explaining points to the furnacemen when using three separate charts. We had available at this time a six-point Kent potentiometric recorder for base-metal thermocouples, calibrated up to 1000° C. The Fery radiation pyrometers were connected to this, and it was found that 300° on this recorder corresponded to 1600° on the crown-temperature indicators. For recording the gas flow a resistance was fixed on the bottom edge of the ring gauge, and the resistance so wound that the square-root scale indicated on this gauge could be converted into a linear scale. To the ends of this resistance a standard e.m.f. was applied, and a chopper bar was arranged to make contact with the resistance at the appropriate time. A diagram of the circuit[5] is shown in Fig. 11. This circuit (including a resistance chosen so that the range of readings falls within a convenient region on the recorder chart) gave a record of the gas flow on the chart of the Kent recorder. A similar arrangement gave the air flow on the same chart. The gas-flow reading is set up to 400 on the Kent recorder chart and extends to 700, and the air-flow reading extends from 700 to 1000.

This gave us a record which had the crown temperature on the left, the gas flow in the middle, and the air flow on the right-hand side of the chart. As standard charts are still being used for

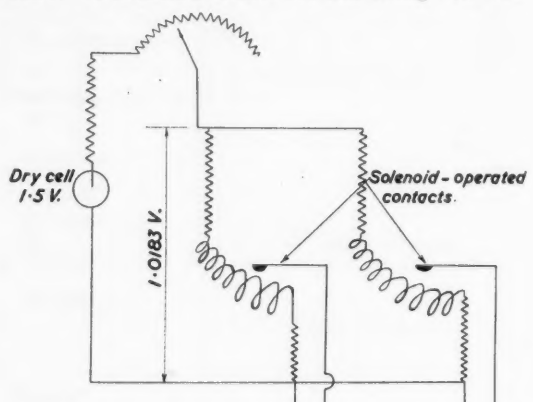


Fig. 11—Circuit diagram for gas and air-flow recorder

this recorder, the gas and air scale has to be written in by hand on the charts before they are used.

The cycle of the recording of crown temperature and gas flow and air flow on the six-point recorder is as follows :

(1) Gas flow.

(2) Crown temperature at the left-hand end of the furnace.

(3) Crown temperature at the right-hand end of the furnace.

(4) Air flow.

(5) Crown temperature at the left-hand end of the furnace.

(6) Crown temperature at the right-hand end of the furnace.

An example of one of these charts is shown in Fig. 12. The crown-temperature scale is very short and could with advantage be extended, but the right-hand side of the record is the maximum temperature, and this edge gives a fairly satisfactory record. The gas- and air-flow records are reasonably clear.

IV—Automatic Crown-Temperature Control

After daily inspection of the combined crown temperature and gas- and air-flow recorder charts for a period, it was obvious that automatic control of the gas valve from the crown temperature would be a decided advantage in certain stages of the melting period.

When a few experiments had been carried out in hand-controlling the gas-regulating valve to definite cycles in order to control the crown temperature, the roof pyrometers were connected to a two-position Kent controller, and, from this, made to operate the motor-driven gas-regulating valve. A timing switch was also incorporated in the circuit so that the regulating valve could be moved for only 4 sec. in every ½ min. A later improvement was the fitting of additional contacts on the Kent controller and also the incorporation of a new timing switch which allowed the gas-regulating valve to open or shut for 4 sec. in every minute if the crown temperature was no more than 10° C. above or below the control figure, and if the temperature was 20° low the rate was 4 sec. in every ½ min. If the temperature reached 20° above normal the valve closed continuously.

For normal operation this control was found to be satisfactory, but under certain conditions, such as (*a*) an empty furnace, (*b*) a furnace ready for tapping, or (*c*) when a furnace door had been open for a considerable period, causing the crown temperature to drop and consequently the gas-regulating valve to open by a large amount, then the control would not shut the gas valve quickly enough to prevent the crown temperature rising unduly. The control is in daily operation and is satisfactory except under the conditions mentioned, and further alterations are now being made in an attempt to overcome these difficulties. Provision is, of course, made for an easy changeover to hand control when necessary.

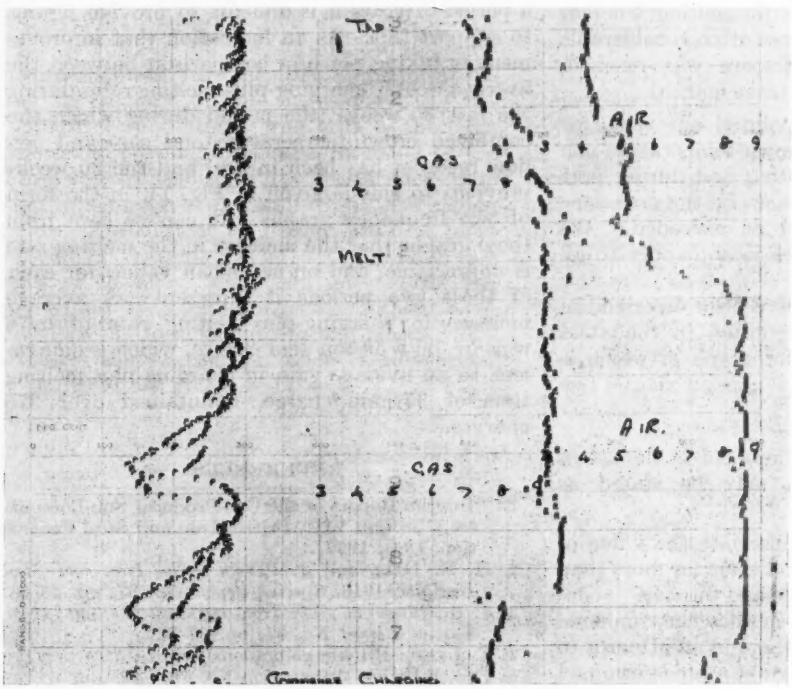


Fig. 12—Crown-temperature and gas- and air-flow recordings on one chart showing (*a*) the drop in crown-temperature when the first and second portion of the charge is added and (*b*) the reduced gas flow after the charge is melted

The advantages to be gained from this crown-temperature control, even in its present experimental stage, are as follows :

(1) During charging and until the control temperature is first reached, a fixed

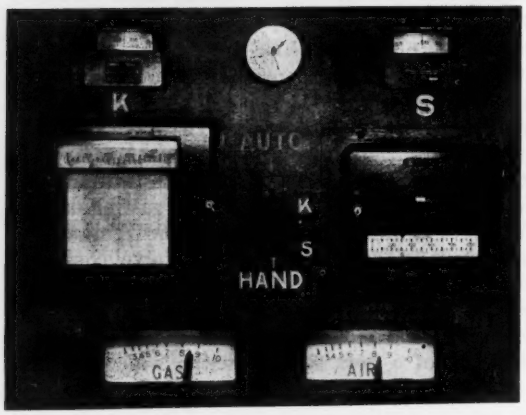


FIG. 13—Latest furnace-control panel

maximum gas flow is maintained, which was not always the case on hand control.

(2) When the control temperature has been reached and the furnace is reversed, the gas valve is opened a considerable amount and is again closed as the temperature reaches normal, and consequently the limiting crown temperature is maintained for a larger proportion of the time.

(3) The necessary temperature for refining is now reached very shortly after melting, whereas with hand control there was often considerable delay before this temperature was reached. This advantage has been most marked.

It should, however, be pointed out that, for our practice, the limiting crown temperature can only be carried during melting and during part of the refining period, as otherwise the necessary tapping temperature would be exceeded. We realize that this condition does not apply to all melting shops.

The instruments which have been described in these notes are now incorporated in the latest furnace-control panel, a photograph of which is shown in Fig. 13.

V—GENERAL RESULTS

The present stage of development in the use of the instruments described may be stated as follows :

(*a*) The furnacemen appreciate the value of the instruments and rely on them far more than on observation of the flame and furnace.

(*b*) Having confidence in these instruments, the furnacemen now use them in an attempt to maintain the furnace crown at a predetermined limiting temperature for as long a period as possible.

(*c*) A maximum flow of gas is maintained for a considerably longer period during charging and melting.

(*d*) Owing to the above factors and the use of immersion pyrometers, the bath temperature is under much better control.

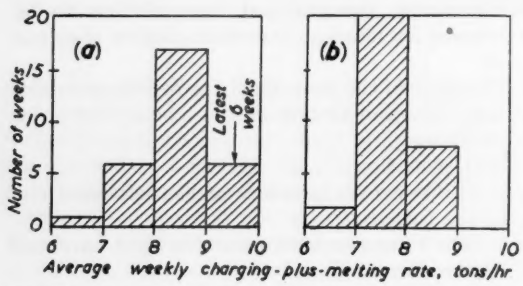


FIG. 14—Frequency graphs of the melting rates (*a*) for 30 weeks during which the combined crown-temperature and gas and air-flow recorder was in use (most frequently occurring rate 8–9 tons/hr., average rate 8·41 tons/hr.) and (*b*) for 30 weeks previous to the use of the combined recorder (most frequently occurring rate 7–8 tons/hr., average rate 7·65 tons/hr.)

The cumulative effect of these results is that there has been an increase in the melting rate, but as the instruments were installed gradually over a period of years it is difficult to provide figures to support this. As an indication that improvement is taking place, a comparison between the average weekly charging-plus-melting rates during the last 30 weeks (the period during which the combined crown-temperature and gas- and air-flow recorder has been in use) and the 30 weeks previous to this is given in Fig. 14, in the form of two frequency graphs. It can be seen from these graphs that the increase in the melting rate is appreciable, and on the mean values for each of these two periods it represents an average increase in charging-plus-melting rate of 0·76 tons/hr. on a 50-ton acid charge, which is equivalent to an average gain in charging-plus-melting time of 35 min./charge, maintained over 376 charges.

REFERENCES

1. " Interim Report of the Gas-Producer Sub-Committee," Section VII. British Iron and Steel Federation, Oct., 1937.
2. H. E. DALL and J. PLUCK : *The Iron and Steel Institute*, 1946, *Special Report No.* 37, pp. 35–38.
3. J. H. CHESTERS : *The Iron and Steel Institute*, 1946, *Special Report No.* 37, pp. 67–72.
4. T. LAND : Private communication.
5. British Patent Application No. 6136/47.

A Micro-Examination of Eight Steels for the Inclusions Sub-Committee*

By J. H. Whiteley, D.Sc., F.R.I.C.†

SYNOPSIS

An investigation for the Inclusions Sub-Committee, of the number, distribution, and nature of the inclusions in a series of eight low-alloy steels is described. In determining the number of inclusions, counts were made on both lengthwise and crosswise surfaces, it having been previously ascertained from the former, that inclusions not well embedded are liable to be removed in polishing. The way this conclusion was arrived at is shown. Full details of the counting procedure are presented. The results indicate a direct relationship between the number of inclusions in the steels and their combined sulphur and oxygen contents. A description of the types of inclusions observed in each of the steels is then given, their grain-sizes and macrostructures being also illustrated. In an appendix by G. Murfitt, an X-ray examination of acid-extracted inclusions in these steels is recorded.

THE investigation here recorded was undertaken at the request of the Inclusions Sub-Committee. Their desire was that a thorough microscopic examination should be made of the non-metallic inclusions in the steels they were testing in a research which aimed at determining the effect of inclusions on fatigue. Ten or more steels, all of the same type but with differing degrees of cleanness, had been selected for the purpose, and samples from eight of them were received in the form of broken test-pieces. The analyses of the steels from which these samples were prepared are given in Table I.

Now that the total oxygen in steel can be accurately estimated, the sum of the sulphur and oxygen contents should provide a good idea of its cleanness, since they are the main inclusion-forming elements. At any rate, the combined amounts ought to afford a safe guidance with

* Paper No. SM/BB/3/46 of the Gases and Non-Metallics Sub-Committee of the Ingots Committee of the British Iron and Steel Research Association, received 6th June, 1947. The views expressed in it are the author's, and are not necessarily endorsed by the Sub-Committee as a body.

† The Consett Iron Co., Ltd.

TABLE I—*Analysis of Samples*

Sample	Ingot Weight, cwt.	C, %	Si, %	Mn, %	S, %	P, %	Cr, %	Ni, %	Mo, %	O_2, %	N_2, %	Class
*BE*1	22	0·31	0·17	0·62	0·007	0·008	0·91	3·76	0·24	0·004	0·010	Basic electric
*BE*2	26	0·29	0·16	0·54	0·026	0·013	0·93	2·96	0·25	0·004	0·008	,, ,,
*BE*3	21½	0·27	0·29	0·60	0·013	0·028	0·82	3·32	0·16	0·002	0·007	,, ,,
*BE*4	21½	0·29	0·29	0·55	0·011	0·033	0·75	3·37	0·23	0·002	0·008	,, ,,
*AO*1	26	0·32	0·17	0·65	0·04	0·023	0·84	3·06	Nil	0·004	0·006	Acid open-hearth
*AO*5	28½	0·32	0·23	0·54	0·031	0·032	0·94	3·56	0·25	0·005	0·004	,, ,, ,,
*AO*8	24	0·32	0·27	0·56	0·042	0·034	1·31	3·82	0·32	0·003	0·005	,, ,, ,,
*BO*1	24	0·30	0·30	0·63	0·024	0·039	0·84	2·37	0·45	0·004	0·005	Basic open-hearth

killed steels, to which class the eight in Table I belong ; for, according to published results, the percentage of oxygen in plain carbon steels of that description, as determined by iodine extraction, agrees well with the vacuum-fusion figure, so that all or nearly all of the oxygen must exist in the inclusions. But while the sulphur and oxygen taken together should indicate the quantity of non-metallic matter to be expected, they can give no information on the average size and the distribution of the inclusions in the steel. At present, that can only be obtained by microscopic examination. It is, in fact, relied on altogether in the Fox and other similar methods for assessing the relative cleanness. The drawback of such methods is, however, that they lack precision, since the results arrived at depend to some extent upon the judgment of the observer. Hence, in dealing with the eight samples, it was felt that a procedure less open to that objection should, if possible, be employed. Quite clearly, an actual count ought to be more trustworthy than a comparison with standard charts, and an attempt was therefore made to develop a workable method by which the average number per unit area of all the inclusions except the extremely fine ones could be ascertained. In so doing it was realized, of course, that no cognizance would be taken of variation in the size of the particles counted, a factor which may be of great importance. Opinion seems to be general that a very large inclusion can be much more harmful than many small ones. Probably by no single method will it ever be possible to present a completely adequate description of the inclusion content of a steel, but a point in favour of counting in dealing with the eight samples was that they had all been cut from the same position in blooms of the same cross-sectional area, these in turn having been rolled from ingots approximately equal in weight.

Before describing the procedure finally adopted for counting, some consideration must first be given to the direction in which the specimen should be cut. In the comparison methods, the observations are usually confined to length sections, but some doubt existed as to whether a true estimate of the inclusion contents of the steels could be obtained in that way. It arose as the result of an investigation which the author began some time ago at the request of the Ingots Committee, on the effect of forging on the distribution of inclusions and, in order to make the reason for the uncertainty clear, a short account of the work which was done will be necessary. Now when thinking over the problem, it seemed likely that any change in distribution would be more readily seen in a steel containing a large number of evenly spaced inclusions, than in one which was relatively clean, and to begin with, therefore, a free-cutting steel appeared to be the most suitable to work with. Accordingly, a block 4 in. sq. by 8 in. long was cut from the axis and about 1 ft. from the base of a 65-cwt. ingot ; that position being chosen with the object of getting a piece in which the inclusions would be of the same average size throughout. The analysis of the steel was as follows :

C, %	S, %	P, %	Mn, %
0·065	0·44	0·048	1·06

Next, to obtain varying degrees of forging, the block was uniformly heated to about 1150° C. and fashioned at one end under the steam hammer into square bars about 6 in. long, representing successively 4, 8, 16, and 32 reductions in area. From these bars and also from the unforged end, lengthwise and crosswise specimens were then prepared for examination.

As was expected, the inclusions in the two specimens of the unforged material appeared to be equal in quantity. They were, of course, very numerous, being chiefly rounded and rather large MnS particles among which were scattered a few silicate globules often containing MnS. A typical example of one of the latter inclusions is shown in Fig. 8. When the lengthwise and crosswise sections of the ×4 reduction bar were compared, however, a distinct difference was noticeable, in that the inclusions in the crosswise section were decidedly smaller on the average and more numerous than in the lengthwise section. The same thing was to be seen in the ×8 reduction sections and it was still more in evidence when the reduction was ×16, while in the ×32 sections the disparity was very pronounced.

These observations plainly indicated that the number of inclusions appearing in the crosswise in excess of those shown by the lengthwise section increased at an accelerating rate with progressive reduction, and in order to demonstrate this divergence a count was made of the inclusions in the unforged ×8, ×16, and ×32 specimens. The procedure was as follows. In each instance a single specimen was used on which a lengthwise and a crosswise polished surface had been prepared. Before counting, the specimen was immersed in 10–15 c.c. of 0·1% sulphuric acid for 10 min., a treatment which sufficed to dissolve the sulphide particles without attacking the iron to any appreciable extent. These inclusions were thus replaced by black cavities which could readily be seen on the ground-glass screen of the microscope camera. On removal from the solution a very light cloth polish was given, after which the specimen was placed on the stage of a horizontal microscope, and a field projected on to the camera screen at a magnification of 50 diameters. The number of visible inclusions in a 1½-in. dia. area was then ascertained, each one, when counted,

TABLE II—*Averages of Inclusions Counts on a Free-Cutting Steel at × 50 Magnification, 1½-in. dia. Circle*

Reduction	Lengthwise Section	Crosswise Section	Difference	Percentage Increase in Cross Section
Unforged	74	72	...	Nil
× 8	60	81	21	35
×16	53	88	35	66
×32	45	180*	135	300

* In this instance a 0·7 in. dia. circle was used, but even so the figure is approximate, since the inclusions were too numerous to count very accurately.

being spotted with ink so as to avoid duplication. Thirty different fields, taken at random on each of the lengthwise and crosswise surfaces of the four specimens, were thus dealt with and the average numbers found. The results are given in Table II. That they were reliable was evident from the fact that the averages of the three successive ten counts on each surface agreed very closely.

At the request of Dr. C. H. Desch, Mr. H. Morrogh of the British Cast Iron Research Association kindly undertook to check some of these results, and accordingly specimens of the × 16 and × 32 reduction bars were sent to him. He reported as follows:

> "The micro specimens were prepared in the usual way, and in the course of this no etching reagent other than picric acid was used, so that the sulphide inclusions were not attacked in any way. Inclusions counts were made at × 50 and × 150 dia. The image was projected on to a ground-glass screen and the number of inclusions in an area 6 × 4 in. was counted. To facilitate this the screen was divided into a number of ruled sections. At × 50 dia. we found the inclusions too numerous in the transverse sections for counting, and so they were only counted at × 150 dia. Ten fields at random were counted in each case."

The results Mr. Morrogh obtained are given in Table III. Like those in Table II they indicate a widening difference between the inclusions to be seen in lengthwise and crosswise sections with increasing reduction.

TABLE III—*Inclusion Counts by Mr. Morrogh on the Free-Cutting Steel at × 150 Dia.*

Reduction	Lengthwise Section	Crosswise Section
×16	78	167
×32	66	314

Mr. Morrogh also took some excellent photographs of the inclusions in the two specimens, four of which are reproduced in Figs. 3–6. A comparison of Figs. 5 and 6, which represent the lengthwise and crosswise surfaces of the × 32 reduction bar, respectively, will show that the inclusions, when viewed in the transverse, appeared to be by far the more numerous. Also, as is shown in Table II, a definite decrease can be seen in the lengthwise surface when Fig. 5 is compared with the corresponding photograph of the × 16 reduction bar (Fig. 3). It should be remarked in passing that the MnS particles were much less elongated in forging than the steel containing them. In the × 32 reduction bar, for instance, the reduction of these inclusions was not above × 10 and often considerably less. On the other hand the silicate inclusions were greatly extended. One such inclusion of several that appeared in the × 8 reduction section is shown in Fig. 7. That it was globular at the outset is practically certain, since all the predominantly silicate inclusions observed in the ingot specimens had, without exception, the form seen in Fig. 8.

Although the elongation of the MnS particles was relatively small, it will undoubtedly account in some measure for the difference between the number of inclusions per unit area shown by the lengthwise and crosswise sections of each bar. Taking an extreme case as an illustration, an area on a transverse section of a rectangular bundle of long rods will include many more rods than the same area in the length direction. Yet that does not seem to be the whole explanation, for it is at least questionable whether the elongation of the MnS inclusions in the × 32 reduction bar was anywhere near sufficient to cause so wide a disparity as is recorded in Tables II and III. A careful comparison seemed to indicate that much of the divergence was due to the number of small inclusions apparent in the crosswise sections becoming increasingly greater than in the lengthwise sections as the reduction proceeded. That this was actually the case was confirmed by placing a glass plate, on which were two parallel lines about ⅛ in. apart, on photographs of the surfaces at × 50 magnification and counting the number of fine spots and very thin threads enclosed by the lines when set normal to the forging direction at ten positions. It was found that the average figure for the crosswise section of the ×16 reduction bar was nearly double that for the lengthwise section. In the crosswise section of the × 32 reduction bar there were approximately five times as many as in the lengthwise. On the other hand, when the relatively large inclusions were counted in the same way the figures for the lengthwise and crosswise sections were in both cases practically the same.

This ascertainment strongly suggested that inclusions which are not well embedded in the surface may be removed during the cloth polishing and, in order to see whether that could in fact occur, the following test was made. A lengthwise surface on a specimen from the × 32 reduction bar was prepared as far as the OO emery paper, after which the surface was polished on Melton broad-cloth impregnated with paraffin wax,* at about 1000 r.p.m. During this operation, liquid soap was used, and about 0·05 g. of alumina, the latter being obtained from the decanted portion of a quantity of purchased levigated material which had been allowed to settle for 20 min. in a 600 c.c. beaker nearly full of water. Four different areas containing several fine threads of MnS were then photographed. Thereupon, the surface was again polished, this time on an unwaxed Melton cloth using water and the same amount of alumina. On comparing the areas with the respective photographs, it was found that in two of them some of the fine inclusions had disappeared completely. One of the areas after the first polish is shown in Fig. 9, and after the second, in Fig. 10.

Another instance of inclusion loss during cloth polishing was seen in a specimen of sample *BO*1. One of several stringers of aluminous inclusions had been selected for illustration, and in an endeavour to eliminate a number of unsightly score marks which started at these inclusions, a further lengthy polish was given using no fresh powder and very little pressure on a cloth which had been well washed in running water. On examination, one of the particles in the stringer was then found to have been dislodged, and a shallow cavity was left. Its position is indicated by the arrow in Fig. 42. After a further similar polish the small unevenness there shown was completely smoothed out.

These observations thus appear to support strongly the suggestion that the differences shown in Tables II and III between the number of inclusions in the lengthwise and crosswise surfaces of the same bar were due in part to a loss, during the final polishing, of fine inclusions in the lengthwise surface. Possibly, also, some of the heavier inclusions which happened to be sectioned very near their base may have likewise been removed. It was decided, therefore, in applying a counting method to the eight samples, in Table I, to ascertain the average number of inclusions per standard area in both lengthwise and crosswise surfaces and then to take the average of the two results as an index of the cleanness of the steel. It was, of course, realized that the figure thus arrived at could only be an approximation, since there was little likelihood that a polishing loss at the lengthwise surface would be either exactly compensated or even or proportionately balanced throughout the series by the undoubted increase in the crosswise surface through particle elongation.

The first requirement in assessing the cleanness of a steel is a well-prepared surface. The inclusions should be all intact and no polishing pits should be present. With the eight samples now under consideration much difficulty was experienced in obtaining that condition. Various procedures were tried, including light and heavy pressures on the Melton cloth, but the surfaces when examined under a $\frac{1}{6}$-in. objective were seldom quite free from small cavities apparently due to the dislodgement of oxide particles. Eventually this trouble was overcome by using a cloth impregnated with paraffin wax in the manner recommended by Nowell.* His description of the method is as follows :

> " About $\frac{1}{4}$ lb. of paraffin wax is melted in a 500 c.c. beaker. The piece of broadcloth is then dipped in the melted paraffin, holding the cloth by the four corners which, because they remain dry, can be used to pull the cloth tightly over the wheel. It is important to warm the wheel under the steam faucet just before covering it with the cloth, as this will prevent the paraffin solidifying in lumps. After the wheel has cooled it is ready for use. A solution of alumina, levigated for 20 min., is suitable, to which has been added a small quantity of liquid soap (necessary to make the solution adhere to the wax surface). The surface of the wheel contains thousands of tiny holes, caused by the way the paraffin freezes in the nap of the cloth. It is these minute holes which hold the alumina particles, thus giving it its excellent cutting quality."

In preparing a specimen the final emery paper used was No. OO, and to remove the scratches a fairly heavy pressure was needed with one or two additions of powder and soap as the polishing proceeded. When once the scratches had been smoothed out to the extent that they could not be seen with a hand lens, the pressure was gradually reduced to a light one, during which time the specimen was rotated through about 90°. It was next etched for 10 sec. in 2·5% nital and given a further light polish. Finally, having ascertained under a $\frac{1}{6}$-in. objective that the surface contained no pits, the specimen was boiled for 10 min. in a solution of 5 g. of picric acid and 25 g. of sodium hydrate in water and again lightly polished. It was then ready for the inclusion count. Hot solutions of both alkaline sodium

* *See* col. 2 for details of this method of polishing.

* L. A. Nowell : *Metal Progress*, 1944, vol. 45, Jan., p. 89.

TABLE IV—*Figures Obtained in 100 Counts*

Sample	Direction of Sample	Etching Reagent	
		Sodium Picrate	0·10% Sulphuric Acid
*AO*1	Lengthwise	1285	1293
*AO*1	Transverse	1448	1436
*BO*1	Lengthwise	1037	1109
*BO*1	Transverse	1934	1923

picrate and sodium hydrate attack sulphide particles so that dark cavities are left which are easy to count. Sodium picrate is preferable, however, because sodium hydrate alone tends to roughen the surface a little.

Since the inclusions in the samples were not very uniformly distributed, especially when viewed in the lengthwise sections, it was obvious that, to obtain a good average of their number, many fields would have to be taken. Also, as fine particles were present in all the samples, the number found would depend on the magnification and possibly on the resolving power of the objective. These considerations, and the fact that in the general opinion large inclusions are likely to be more deleterious than small ones, led to the adoption of the fairly low magnification of × 45 dia. after both higher and lower powers had been tried. With the × 5 eyepiece used, the field was then 2 mm. in diameter and needed to be reduced, since the inclusions in an area of that size were often too numerous for easy and accurate counting. An opaque disc with a central hole 1·5 mm. dia. was therefore inserted in the ocular, by which means a field of exactly 0·3 mm. dia. was obtained. A typical example is given in Fig. 11 which reproduces, as nearly as possible, the actual size of the field as observed. The source of light was a 100-candle-power Pointolite lamp. No colour filter was used and, to avoid eyestrain, a ground-glass screen was placed in front of the vertical illuminator. Only black spots which were well defined after careful focusing were counted. Occasionally an extremely fine one which did not quite meet that condition would appear, and there was then some uncertainty as to whether or not to include it. Various attempts were made to eliminate such spots without affecting the clearness of the rest, but no satisfactory method was found. The error due to these insufficiently distinct inclusions would, however, not exceed 5%.

Having decided on the magnification and size of field to work with, the procedure in counting was as follows. First, the number of inclusions was ascertained in 100 fields on a surface that had been treated with sodium picrate. Then, to check this result a second 100-field count was made after the specimen had been immersed in about 10 c.c. of 0·10% sulphuric acid for 30 sec. and again lightly polished. This acid solution, although so weak, dissolves both iron and manganese sulphides much more rapidly than does boiling sodium picrate, and was found to be equally safe to use for sulphide removal in the short time given. In most cases the agreement between the two counts was of the order shown in Table IV.

In one or two instances where the difference exceeded 100, recounts were made on a new surface and the average for the 400 then obtained. The results multiplied by 10 are presented in Table V. The sixth column contains the averages of the combined lengthwise and transverse figures, and the seventh column the average number of observed inclusions per square millimetre of surface as calculated from the sixth column data. The seventh column figures are plotted in Fig. 1 against the total percentages of sulphur and oxygen.

Although the points in Fig. 1 are rather scattered, a direct relationship is indicated between the cleanness of the steels and the amounts of the two inclusion-forming elements in them, the correlation coefficient being +0·88.

TABLE V—*The Results of the Inclusion Count*

Sample	Sulphur, %	Oxygen, %	Average Results × 10		Average	Average/sq. mm.	Longest Inclusion Observed, mm.
			Lengthwise	Transverse			
*BE*1	0·007	0·004	46	66	56	79	0·28
*BE*2	0·026	0·004	80	134	107	151	0·45
*BE*3	0·013	0·002	69	109	89	125	0·14
*BE*4	0·011	0·002	66	83	75	105	0·30
*AO*1	0·040	0·004	129	144	136	192	0·50
*AO*5	0·031	0·005	107	140	124	174	0·85
*AO*8	0·042	0·003	166	231	198	279	1·40
*BO*1	0·024	0·004	107	193	150	211	0·60

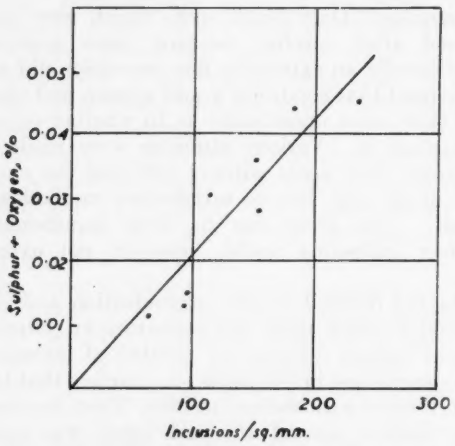


FIG. 1—Average number of inclusions per sq. mm., seen at × 45 magnification in lengthwise and crosswise surfaces, plotted against the sulphur-plus-oxygen content of the steel

For several reasons a very close alignment of the points is scarcely to be expected. In the first place there are the different combining proportions of the oxygen, as for example in MnO, Al_2O_3, and SiO_2. Sulphur may likewise cause a slight disturbance if small amounts of chromium or molybdenum sulphide are present. Again, the average inclusion sizes in the eight steels may not be identical even though the blooms were all rolled from ingots of practically equal dimensions. It was noticed that very small particles were more prevalent in sample *AO*8 than in the rest. Then too, the extent to which the oxides and sulphides were associated varied widely; in sample *AO*1, for instance, not many inclusions were duplex,

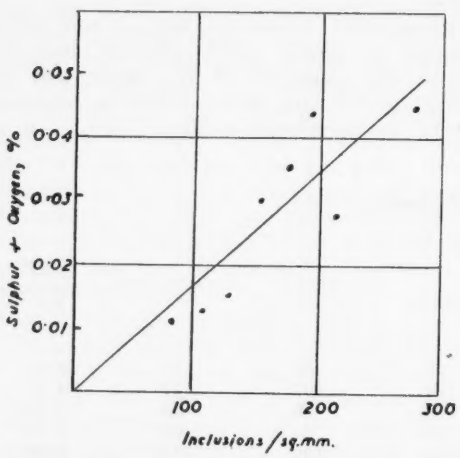


FIG. 2—Average number of inclusions per sq. mm. seen at × 45 magnification in the lengthwise surfaces only, plotted against the sulphur-plus-oxygen content of the steel

whereas most of them were of that description in sample *AO*5. Also, as already mentioned, the average of the lengthwise and transverse counts can only be regarded as an approximation to the actual number of inclusions per unit area. It may be that for ordinary purposes the usual procedure of taking only lengthwise sections affords a good indication of the relative cleanness of the steel. Certainly the curve shown in Fig. 2, obtained by plotting the lengthwise counts alone (per sq. mm.) against the combined sulphur and oxygen contents, compares very favourably with that of Fig. 1, for its correlation coefficient is + 0·86. Before the question can be finally settled, however, further similar work may be needed on a much larger number of steels.

In order to give a general idea of the size and distribution of the inclusions in the steels, low-power photographs of fairly representative areas in both lengthwise and transverse surfaces were taken after the specimens had been boiled for 10 min. in alkaline sodium picrate. They are reproduced in Figs. 12 to 27. These illustrations, like the counting results in Table V, clearly show sample *BE*1 to be the cleanest steel. The length of the longest inclusion observed in the several sections examined of each steel is stated in the last column of Table V. In one or two cases the inclusion was MnS and the remainder were either stringers of aluminous particles or else large inclusions of the kind shown in Fig. 28. The latter type was encountered in all four basic electric steels, and was especially prevalent in sample *BE*4. Instances are to be seen in Figs. 18, 19, and 45. Such inclusions were usually angular and complex, being, most probably, refractory particles picked up during the casting operation. In the author's experience basic electric steels are more liable to contamination with that sort of heavy inclusion than open-hearth steels.

Some of the smaller oxide particles in the steels may have had the same origin, but there can be little doubt that the majority of them were derived from the residual FeO in the bath when tapped. Occasionally, these inclusions occurred in the form of stringers, as shown, for instance, in Fig. 40, but usually they were distributed at random and were often associated with MnS. As far as could be judged from microscopic examination, inclusions apparently due to deoxidation, and which will therefore be termed oxide, were of a similar character in all the steels. Particles high in alumina seemed to predominate, whilst glassy silicates readily soluble in hydrofluoric acid were also present. As was to be expected from the class of steel to which the samples belonged, neither FeO nor MnO was seen as a separate constituent. Having made these general remarks on the inclusions, a short description will now be given of the

FIG. 3—Inclusions in a 0·45%-sulphur steel bar of ×16 reduction Longitudinal section ×50

FIG. 4—Inclusions in a 0·45%-sulphur steel bar of ×16 reduction Cross-section ×50

FIG. 5—Inclusions in a 0·45%-sulphur steel bar of ×32 reduction Longitudinal section ×50

FIG. 6—Inclusions in a 0·45%-sulphur steel bar of ×32 reduction Cross-section ×50

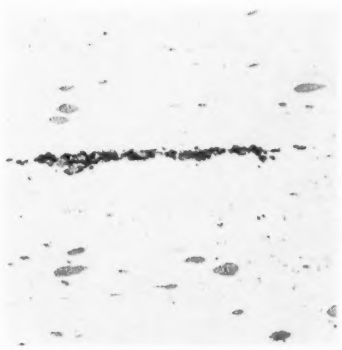


FIG. 7—Elongated silicate inclusion in a 0·45%-sulphur steel bar of ×8 reduction Cross-section ×80

FIG. 8—Silicate globule containing MnS in 0·45%-sulphur steel ingot ×330

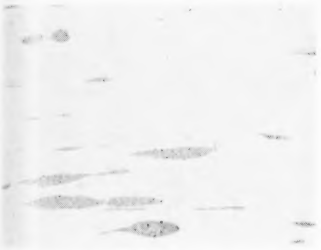


FIG. 9—Inclusions in 0·45%-sulphur steel bar ×125

FIG. 10—Same as Fig. 9 after cloth repolish only ×125

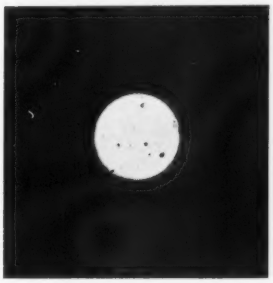


FIG. 11—Showing a field in the inclusion count; actual size as observed ×45

FIG. 12—Inclusions in sample No. *BE* 1. Longitudinal section, sodium picrate etch ×25

FIG. 13—Inclusions in sample No. *BE* 1. Cross-section, sodium picrate etch ×25

FIG. 14—Inclusions in sample No. *BE* 2. Longitudinal section, sodium picrate etch ×25

[*Whiteley*
[*To face p. 94*

FIG. 15—Inclusions in sample No. *BE* 2. Cross-section, sodium picrate etch ×25

FIG. 16—Inclusions in sample No. *BE* 3. Longitudinal section, sodium picrate etch ×25

FIG. 17—Inclusions in sample No. *BE* 3. Cross-section, sodium picrate etch ×25

FIG. 18—Inclusions in sample No. *BE* 4. Longitudinal section, sodium picrate etch ×25

FIG. 19—Inclusions in sample No. *BE* 4. Cross-section, sodium picrate etch ×25

FIG. 20—Inclusion in sample No. *AO*1. Longitudinal section, sodium picrate etch ×25

FIG. 21—Inclusions in sample No. *AO*1. Cross-section, sodium picrate etch ×25

FIG. 22—Inclusions in sample No. *AO*5. Longitudinal section, sodium picrate etch ×25

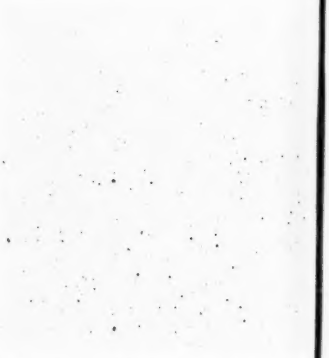


FIG. 23—Inclusions in sample No. *AO*5. Cross-section, sodium picrate etch ×25

FIG. 24—Inclusions in sample No. *AO*8. Longitudinal section, sodium picrate etch ×25

FIG. 25—Inclusions in sample No. *AO*8. Cross-section, sodium picrate etch ×25

FIG. 26—Inclusions in sample No. *BO*1. Longitudinal section, sodium picrate etch ×25

FIG. 27—Inclusions in sample No. *BO* 1. Cross-section, sodium picrate etch ×25

FIG. 28—Large refractory inclusion in sample No. *BE* 3 ×520

FIG. 29—Glassy inclusions soluble in HF in sample No. *BE* 1 ×880

FIG. 30—Inclusion insoluble in HF in sample No. *BE* 1 ×520

FIG. 32—Inclusion with MnS in sample No. *BE* 1 ×880

FIG. 33—Crystal in cavity left by solution of glassy inclusion in HF. Sample No. *BE* 1 ×520

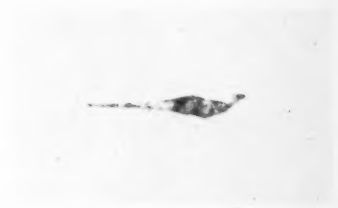


FIG. 34—Composite glassy inclusion in sample No. *BE* 1 ×520

FIG. 31—Hexagonal inclusion insoluble in HF in sample No. *BE* 1 ×780

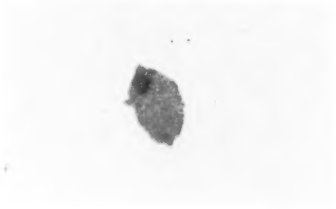


FIG. 36—Inclusion in sample No. *BE* 3 ×650

FIG. 37—Glassy inclusion with MnS, soluble in HF, in sample No. *BE* 3 ×330

FIG. 38—MnS containing aluminous inclusions in sample No. *BE* 4 ×780

FIG. 35—Stringer of cindery aluminous inclusions in sample No. *BE* 2 ×330

FIG. 39—Complex inclusion in sample No. *BE* 4 ×330

FIG. 40—Stringer of aluminous inclusions in sample No. *AO* 1 ×330

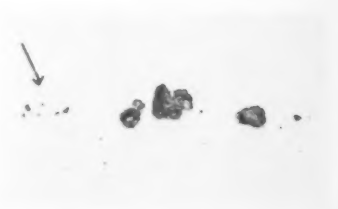


FIG. 41—Aluminous inclusions in sample No. *AO* 8 ×330

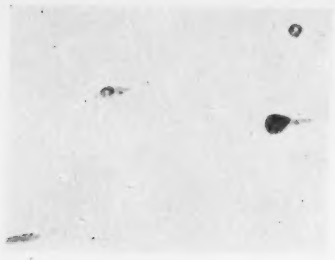


FIG. 42—Typical inclusions in sample No. *AO* 5 ×520

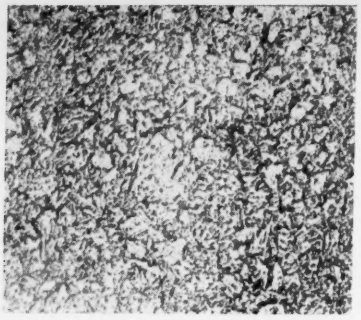


FIG. 43—Showing mixed grain-sizes in sample No. *BE* 1. ×250

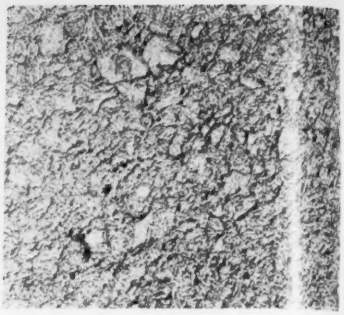


FIG. 44—Showing grain-size in sample No. *BE* 2 ×250

FIG. 45—Showing grain-size and refractory inclusion in sample No. *BE* 4 ×250

FIG. 46—Banded structure of sample No. *BE* 1. Cupric etch ×18

FIG. 47—Banded structure of sample No. *BE* 2. Cupric etch ×18

FIG. 48—Banded structure of sample No. *BE* 3. Cupric etch ×18

FIG. 49—Banded structure of sample No. *BE* 4. Cupric etch ×18

FIG. 50—Banded structure of sample No. *AO* 1. Cupric etch ×18

FIG. 51—Banded structure of sample No. *AO* 5. Cupric etch ×18

FIG. 52—Banded structure of sample No. *AO* 8. Cupric etch ×18

FIG. 53—Banded structure of sample No. *BO* 1. Cupric etch ×18

types observed in any quantity in each of the steels taken in turn.

Steel No. BE*1*

Very little MnS was to be seen. Also, the oxide inclusions seemed few in number in this steel as compared with some of the other samples with the same or lower oxygen contents. These inclusions were either glassy, as shown in Fig. 29, or they consisted of very small idiomorphic and almost opaque crystals of the type shown in Fig. 30. The latter kind were unattacked by hydrofluoric acid and in some instances, particularly in the crosswise sections, they were hexagonal in shape, thus suggesting corundum, although that could not be definitely established. An example is shown in Fig. 31. Occasionally the oxide inclusions were associated with MnS, as in the instance given in Fig. 32. The glassy inclusions were readily attacked by hydrofluoric acid. Immersion of the surface for 1 min. in the concentrated acid removed them completely, and hence their chief constituent was most probably silica. A cavity due to the solution of one of these particles is shown in Fig. 33. In it there remained an unattacked idiomorphic inclusion, of the variety illustrated in Figs. 30 and 31, which was not noticeable before the etch. Another composite particle of the same kind is shown in Fig. 34. No stringers of inclusions were found.

Steel No. BE*2*

There was much more MnS in this steel, and a few stringers of aluminous inclusions were to be seen. One of the longest is partly shown in Fig. 35. The particles of the stringer had a cindery appearance and contained numerous, minute, glassy spherules. Separate small inclusions of this material also occurred. Infrequently, a glassy inclusion was observed. Sometimes it was associated with MnS in the manner shown in Fig. 37. These glassy particles were attacked by strong hydrofluoric acid as was to a lesser extent the ground mass of the cindery substance, but the tiny spherules remained unaffected. Quite a number of minute idiomorphic inclusions were present which were probably crystalline alumina. They were unattacked by hydrofluoric acid and were often situated within MnS inclusions ; such composite particles resembling that shown in Fig. 38.

Steels Nos. BE*3 and* BE*4*

These two steels were very much alike. They both contained at least three kinds of oxide inclusions. Some were idiomorphic, semi-opaque, and unattacked by hydrofluoric acid. In a few instances a definitely hexagonal shape was observed ; in fact, these inclusions seemed to be of the same nature as the idiomorphic crystals encountered in steel *BE*1 which are illustrated in Figs. 30 and 31. One of the largest-size particles is shown in Fig. 36. Then a very glassy inclusion was occasionally found which was readily soluble in strong hydrofluoric acid. An example is shown in Fig. 37. Both types of inclusion were often associated with MnS, small particles of which can be seen at the side of the inclusion in Fig. 37. In the illustration given in Fig. 38, the MnS has within it three of the idiomorphic inclusions. The third kind of inclusion had a cindery appearance ; in it minute glassy spherules were embedded, as was the case with the very similar material observed in steel *BE*2 and shown in Fig. 35. The complex particle in Fig. 39 consisted mainly of this inclusion and the glassy sort. These two steels, like *BE*1, seemed to be practically free from aluminous inclusions in stringer formation.

Steel No. AO*1*

The MnS inclusions were definitely more numerous in this steel than in the previous four, and were for the most part unassociated. The oxide inclusions were of the type generally described as aluminous, consisting of very hard translucent nodules not attacked by concentrated hydrofluoric acid, and sometimes in stringer formation. Part of the longest stringer observed is shown in Fig. 40. In polishing the surface these inclusions gave rise to deep score marks in the adjacent metal. There were also present a few fine elongated particles of a light glassy material of the kind seen in Fig. 34. They were usually associated with MnS and were soluble in hydrofluoric acid. Since the aluminous particles predominated there can be little doubt that the oxide inclusions were chiefly alumina and aluminium silicate.

Steel No. AO*5*

This steel contained about the same quantity of MnS inclusions as the previous one, but it differed in that nearly all of them were associated with bright glassy spherules or else with a dark semi-opaque substance which occurred either as rounded particles or as small idiomorphic crystals. These two types of oxide inclusion, one of each associated with MnS, are shown in Fig. 41. The glassy particles appeared to be rich in silica since they were readily attacked by hydrofluoric acid ; the darker inclusion was more resistant and could not be identified. Stringers of aluminous nodules of the type illustrated in Fig. 40 were not found, neither were separate particles of that material.

Steel No. AO*8*

In every prepared lengthwise surface of this steel three or four stringers and elongated groups of aluminous particles were to be seen, the largest observed being 1·4 mm. long. An example is

given in Fig. 42 which was taken after the specimen had been lightly polished for several minutes in order to smooth out, as far as possible, the deep score marks so difficult to avoid with that type of inclusion. Isolated nodules of this substance also occurred, and a few dark minute idiomorphic crystals associated with the MnS were seen, but the latter was mostly unassociated.

Steel No. BO*1*

This steel likewise contained some stringers of aluminous nodules of the type shown in Fig. 42. Occasionally a single particle was encountered. The MnS inclusions were frequently associated with minute idiomorphic crystals in a similar way to that shown in Fig. 38. Many of these minute crystals had a reddish tinge, especially at the margins ; when viewed in white light some of them were very translucent and bright red in colour.

Since the chief aim of the Inclusions Sub-Committee was to ascertain the effect of inclusions on fatigue, it appeared advisable to examine, during the course of this microscopic investigation, two other variables which might have an influence, *viz.*, the grain-sizes of the test-pieces and their banded structures. Now the grain boundaries of alloy steels such as these are not delineated by the ordinary etching reagents, especially when the steel is in the quenched-and-tempered condition, and consequently some way of revealing them had to be found. In considering this problem it seemed likely that an extremely slow etch might answer, since this method had previously given excellent results with plain carbon steels. Some tests were accordingly made with very dilute mixtures in different proportions, of hydrochloric, nitric, and picric acids in alcohol, but none were successful. Eventually, however, a fairly satisfactory result was obtained with the reagent originally employed, *viz.*, 0·6 g. of picric acid dissolved in 100 c.c. of 95% absolute alcohol. With this solution the attack was never localized and no pitting occurred, as sometimes happened when the less pure industrial alcohol was used.

The etching procedure was as follows. A polished transverse surface was prepared on a section, about 5·0 g. in weight, of the ½-in. dia. test-piece. The specimen was then placed, with the polished face upwards, in a porcelain capsule of 10 c.c. capacity with about 4·0 c.c. of the above reagent, which was amply sufficient to immerse the piece completely. Thereupon the crucible was covered and left undisturbed for 18 hr. During that time a black deposit gradually formed on the surface which was easily removed by little more than a momentary touch on the revolving wet cloth after the specimen had been washed. The surface was then examined under the microscope at about × 250 magnification, and if the grain boundaries were not very clearly defined a further very slight polish was given. Often a third touch on the cloth was needed in order to bring out the structure to the best advantage. Occasionally the polishing was overdone, in which case the surface was re-etched for a further 18 hr. A double etch with an intermediate polish was, in fact, sometimes preferable. This method of etching not only revealed the grain boundaries but also the needle-like quenching structure and, in addition, the dendritic macrostructure was strikingly evident in several of the sections, especially when viewed under a hand lens. Examples showing the grain boundaries and the needles within the grains are given in Figs. 43, 44, and 45. A mixed grain is apparent in Fig. 43. The grain-sizes of the other five test-pieces were within the limits seen in these illustrations.

Lastly, the banded structures of the steels were obtained by means of Rosenhain and Haughton's reagent* in which the polished lengthwise surfaces were dipped for 3–4 sec. Typical areas are shown in Figs. 46 to 53. Similar structures were developed by Le Chatelier and Dupuys' reagent.†

Appendix—*X-Ray Examination of Acid-Extracted Non-Metallic Inclusions*

By G. Murfitt, A.Met., F.I.M., F.R.I.C.‡

The method of examination of inclusions, described in this appendix to Dr. Whiteley's paper, is an extension of the method according to Dickenson.§ The procedure for clean steels is to take a much larger weight of sample than the 75 g. recommended by Dickenson. An initial sample of 1500–2000 g. of a clean steel will yield a residue of 0·1 g., approximately. The larger sample is cut into several small pieces which are carefully ground to remove surface scale, and then dissolved in cold dilute nitric acid (1 : 9). The residues are treated as in the Dickenson procedure ; the only modification being the use of 5% sodium carbonate solution instead of the 10% sodium hydroxide solution, to remove gelatinous silicic acid. The residues and filters are combined, ignited at a low temperature, and weighed. The residue is then analysed as a normal silicate. It was found advisable to use platinum apparatus as far as possible, especially for evaporation and baking.

Unfortunately, only five of the eight steels examined by Dr. Whiteley were available in

* W. Rosenhain and J. L. Houghton, *Journal of the Iron and Steel Institute*, 1914, No. I, p. 515.

† H. Le Chatelier and E. L. Dupuy, *Comptes Rendus*, 1917, vol. 165, Sept. 10.

‡ Messrs. Wm. Jessop & Sons, Ltd.

§ J. H. S. Dickenson : *Journal of the Iron and Steel Institute*, 1926, No. I, pp. 177–211P.

TABLE VI—*Constitution of Acid-Extracted Non-Metallic Inclusions*

Sample	Constituents Observed	Proportions Present	Remarks
*AO*5	TiO_2 (rutile) SiO_2 (quartz) SiO_2 (cristobalite) $3Al_2O_3 . 2SiO_2$ (mullite)	Medium–large Small–medium Small–medium Small	
*AO*8	Fe_2O_3 (ferric oxide) α–Al_2O_3 (alumina)	Large Small	The two R_2O_3 phases are independently present, not in partial solid solution
*BO*1	α–Al_2O_3 (α alumina) γ–Al_2O_3 (γ alumina) Garnet : (probably) pyroxene of enstatite - type FeO type of solid solution, possibly (Fe.Mg)O	Large–medium Medium Small–medium	The garnet, according to its lattice parameter, would agree with pyrope, $3(Fe.Mg)O.Al_2O_3 . 3SiO_2$ which, however, does not necessarily imply the presence of the Mg ion, as a considerable width of substitution exists in the garnet family, also involving Mn, Ca, and Ti. The same applies to the pyroxene phase observed (Mg. Fe)O.SiO_2 and the FeO phase.
*BE*3	Spinel solid solution SiO_2 (quartz) Fe_2O_3 (ferric oxide) Pyroxene of enstatite type	Large Small Small Small–medium	The spinel, according to its lattice parameter, corresponds to a $MgO.Al_2O_3$–γ-Al_2O_3 solid solution. Fe** ions may also have entered this spinel, but the presence of Mn** and Cr*** is improbable.
*BE*4	SiO_2 (quartz)	Pure	This conflicts with the statement after microscopic examination, that an 'aluminous' inclusion was present. A check X-ray photograph (fresh sample) confirmed, however, quartz.

sufficient quantity, namely, *AO*5, *AO*8, *BO*1, *BE*3, and *BE*4. The pieces supplied were broken test-pieces, each weighing approximately 120 g. These were treated according to the modified Dickenson procedure and yielded residues of the order of 0·01 g.

The constitution of the inclusions was determined by X-ray analysis of the residues, using the powder method and the 9-cm. diameter Debye–Scherrer camera. A considerable number of inclusions had been subjected to X-ray examination previously, so that the technique was established; the required standard compounds were available as the outcome of earlier research on refractories, slags, and non-metallic inclusions. Rait and Pinder* have reported the results obtained by X-rays on previous non-metallic inclusions in steels extracted chemically or mechanically, and reference may be made to this paper for particulars.

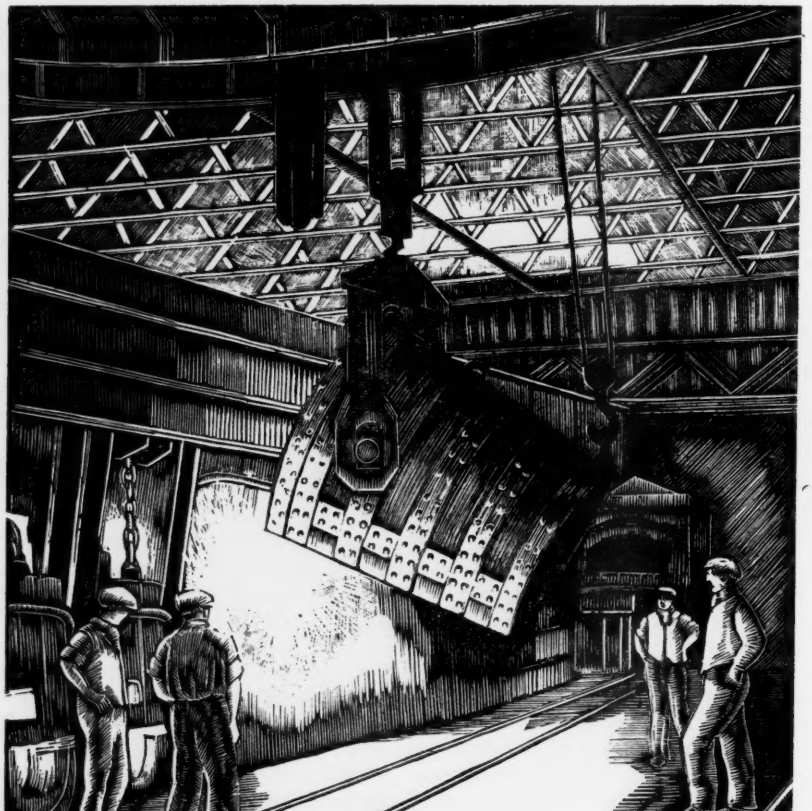
The constitutions found are summarized in the following Table VI, with some comments.

* J. R. Rait and H. W. Pinder: *Journal of the Iron and Steel Institute*, 1946 No. II, pp. 371–398.

It should be mentioned that of course only crystalline phases are observed by the X-ray method, and that any amorphous constituents which are quite liable to occur would escape detection. Furthermore, for phases with poorly developed crystallinity such as cristobalite, after certain conditions of cooling, the lower visibility limit may be very appreciable; up to 20% may not be recorded in unfavourable cases, against the limit of ½% for well-crystallized phases, *e.g.*, quartz and α alumina, with their intense reflections. For this, among other reasons, it would be misleading and incorrect to attempt a quantitative determination of amounts from the X-ray pattern, but the terms such as large, medium, and small, given in the Table may serve as an approximate guide.

ACKNOWLEDGMENTS

The author wishes to thank Mr. D. A. Oliver, Director of Research, Messrs. Wm. Jessop & Sons, Ltd., for his interest in the work, and Messrs. E. W. Harpham and H. J. Goldschmidt, who were responsible for the chemical extractions and X-ray examinations respectively.



The above illustration, from the original woodcut by Viva Talbot, shows hot metal being poured into an open-hearth furnace.

DISCUSSION AT

A Combined Meeting on Side-Blown Converter Practice

By arrangement between the British Iron and Steel Research Association and the Iron and Steel Institute, a meeting, at which members of the Institute of British Foundrymen were invited to be present, was held at the Offices of the Iron and Steel Institute on Thursday, 20th March, 1947. It had been hoped that Professor J. H. ANDREW, of Sheffield University, would take the Chair, but in his unavoidable absence Mr. F. COUSANS, of Messrs. Catton & Co., Ltd., Leeds, deputized for him. There were two sessions, starting at 10.30 A.M. and 2.15 P.M.; during the interval a Buffet Luncheon was served.

AT THE MORNING SESSION Dr. (now Professor) R. J. SARJANT presented the "First Report of the Converter Refractories Sub-Committee" of the British Iron and Steel Research Association.

AT THE AFTERNOON SESSION the "First Report of the Side-Blown Converter Practice Sub-Committee" of the Association was presented by Dr. T. P. COLCLOUGH.

PROCEEDINGS OF THE MORNING SESSION: 10.30 A.M. to 12.45 P.M.

The **Chairman**, in opening the meeting, said: It is to be regretted that, owing to indisposition, Professor J. H. Andrew, of Sheffield University, is unable to be here today, and to occupy the chair. He is Chairman of the Steel Casting Panel of the British Iron and Steel Research Association. In his absence, it has fallen to me to take over his duties for the day, and it is my very pleasant task to welcome Mr. D. Howard Wood, the President, and the members of the Institute of British Foundrymen to this meeting of the Iron and Steel Institute. Members of the Main Technical Committee of the British Steel Founders' Association and of the British Iron and Steel Research Association have also been invited to attend and are represented here today. I feel sure that you would wish me to welcome them to this meeting, and to express the hope that they will take part in the discussion.

The Institute of British Foundrymen have stimulated considerable interest in the use of basic cupolas, and have been responsible for sponsoring work and research in connection with desulphurizing and dephosphorizing cupola-melted metal. Work of this nature is complementary to the work which the British Iron and Steel Research Association and the Iron and Steel Institute are endeavouring to do in connection with the use of side-blown converter plants for steel-casting production. It is, therefore, particularly fitting that their President and members should be present today, when two Converter Reports are given by the respective Sub-Committee Chairmen, Professor Sarjant and Dr. Colclough.

During the recent war, the operators of side-blown-converter steel plants were faced with many problems arising from the vital necessity of reducing to a minimum the use of hematite pig iron. It is now well known that during the war the country was at one period in a very difficult position as regards supplies of hematite pig iron, and, accordingly, efforts were made by the technical staff of the Iron and Steel Control to assist steel founders with their side converter practice by the provision of plant and of technical knowledge.

Many of the converter foundries operating on cupola-melted metal found that their cupola installations were of very old design and gave metal of low total carbon content, so that when asked to use up to 80% of steel scrap in their cupola charge they found themselves in a very difficult position in obtaining a satisfactory temperature increment during the subsequent conversion process.

We are conversant with the remarkable results obtained at the Stanton Gate Foundry during the war, in which cupola-melted metal of low silicon content was superheated in pulverized-coal fired furnaces and finally converted in side-blown Bessemer converters. Steel made in this manner possessed excellent fluidity and gave excellent physical properties after proper heat-treatment.

Three or four firms, during the period of the war, had been carrying out research work on their Bessemer steelmaking, with the object of seeing what improvements could be effected, and the Control brought together representatives of four firms and suggested that they pool their knowledge and experience with the object of making it available for the benefit of all converter users. This meeting was held in September, 1943, and a Committee was formed under the chairmanship of Dr. Colclough. The constitution of the Committee is given in the Report, and, as one who was privileged to know something of the work of this Committee, I should like to take advantage of the present opportunity to pay my tribute to the hard work and enthusiasm of the Chairman, Dr. Colclough, and of the Secretary, Mr. Spray of the Stanton Ironworks.

The Directors of the firms who had been associated with this work have been extremely generous in allowing the information to be published, and the two documents which we are going to have the opportunity of discussing today are extremely valuable.

This is the first time, so far as I know, that so many data have been published on the subject of British side-blown converter practice.

A Refractories Sub-Committee was formed under the chairmanship of Professor Sarjant (then of Messrs. Hadfields, Ltd.), with the assistance of representatives from the larger steel-producing firms possessing research laboratories, together with other refractory technologists. I wish to take advantage of this opportunity to congratulate the Chairman of the Converter Refractories Sub-Committee, Professor Sarjant, on his appointment to the Chair of Fuel Technology at the University of Sheffield. (*Applause.*)

Professor Sarjant and the members of his Sub-Committee have done a most valuable piece of work, first in surveying the general current practice, and then in proceeding to carry out an examination of converter lining materials. You will see from the Report that they have examined, in a detailed way, monolithic linings as well as brick linings, and they have given some attention to experimental work in connection with the drying and pre-heating of converters, and have made a study of silica-brick linings.

On behalf of the Iron and Steel Institute, I should like to tender an apology for the inability of the Institute to put the Report of the Converter Refractories Sub-Committee in your hands at an earlier stage, but owing to the present national emergency the printing position has been made extremely difficult.

I call on Professor Sarjant to present the "First Report of the Converter Refractories Sub-Committee."

Discussion of the FIRST REPORT OF THE CONVERTER REFRACTORIES SUB-COMMITTEE,*

of the Moulds Research Committee

Professor **R. J. Sarjant** (Sheffield University): I should like to thank the Chairman for his very kind references to me personally, and to thank all of you for your kind acceptance of those congratulations, which I value very highly, coming from my old colleagues in the iron and steel industry. I hope that my activities will still keep me closely associated with the industry, although perhaps my field of activity, in a metallurgical sense, will not be so wide. We are intending in Sheffield to make the Department of Fuel Technology of real service to the iron and steel industry. We aim at making it a department to which the industry will look for well-trained men, and particularly we want to develop a strong research school. It may take some years to achieve all our objects, because it is difficult to get men, plant, and equipment at the moment, but there is an ambitious scheme of development which we hope will commend itself to the industry. I hope, still, to be amongst you and to be looked upon by you as one of your colleagues and friends.

I would direct your attention particularly to the seven skittles, on page 269, which you may attempt to knock down, and which, if you have come here intending to discuss this paper, you will have particularly noted. It should be added that since the Report was written, a full series of comparative trials is being run by Messrs. Catton & Co., Ltd., and that work is now well under way. The members of the Sub-Committee are particularly indebted to Messrs. Catton & Co., Ltd., for the whole-hearted enthusiasm with which they have co-operated in those investigations.

Mr. **S. Spray** (Stanton Ironworks Co., Ltd.) and Dr. **A. H. B. Cross** (The Brown-Firth Research Laboratories) gave an outline of the contents of the Report.

* *Journal of the Iron and Steel Institute*, 1947, vol. 155, Feb., pp. 235–269.

DISCUSSION

Mr. **D. Howard Wood** (President, the Institute of British Foundrymen): I hesitate to take up even one moment of the short amount of time left for discussion, but there are two things that I should like to say. First, I want to express on behalf of the members of the Institute of British Foundrymen, our grateful thanks to the Iron and Steel Institute and to the British Iron and Steel Research Association for their kindness in permitting us to participate in this combined meeting. I think about a third of those present are members of the Institute of British Foundrymen, so that we have taken advantage of your kindness. My Institute will be happy to reciprocate by inviting your members to be present on any suitable occasion when papers of interest to you are discussed. We are to have a Conference in June, in Nottingham at which a paper on side-blown converter practice will be presented by yourself, Mr. Chairman. At that Conference, also, Mr. Finch is giving a paper on feeding steel castings, while our President-Elect, Mr. P. H. Wilson, is describing the Stanton Gate Foundry.

Often, when travelling to and from business, my eye is caught by the slogan, "Co-operation, the Hope of the Worker." I should like to modify that, and say, "Co-operation, the Hope of Industry." I am referring, of course, to research and development, not to party politics. I think that the Report presented this morning offers an excellent example of co-operation in industry, because I see that some twenty-two firms have co-operated in the experimental work on which the Report is based.

At the Joint Meeting yesterday,* Dr. Colclough

* Joint Meeting on Desulphurization and Dephosphorization of Cupola Iron and Pig Iron, Wed., 19th March, 1947.

emphasized the importance of the application of research and development to industry. I feel that if some means could be found for doing away with the time-lag, and speeding up the putting into practice in industry of the discoveries which are made, it would be of tremendous advantage to industry.

In again thanking you for inviting the members of my Institute to be present, I would express the hope that the remainder of your Conference will be as successful as the Joint Meeting.

Dr. **J. White** (Sheffield University) : Since we shall be concerned at Sheffield with the continuation of this work under the new Committee, we are naturally keenly interested in the present report. It may be of interest to comment on certain points emerging from the Report in the light of the later work, which up to the present has been concerned with the examination of monolithic linings.

Dealing first with the question of the grading of monolithic linings, the materials described in the Report show very wide differences in grading both in respect of actual particle size and of size distribution, and it is clear that no generally accepted optimum grading is recognized for this application. This agrees with the results of our own examination of lining materials, and we feel that trials of material graded according to recognized principles, and with probably lower clay contents than are generally used, would be well worth while. There are, of course, difficulties in the way of milling mixtures of the present type to produce the ideal type of grading.

Our results up to the present would indicate that improved physical properties would result from the use of properly graded material. The optimum size distribution in a continuously graded batch has been established on theoretical grounds, and is given by the equation :

$$P_x = 100\left(\frac{x}{D}\right)^m$$

where P_x is the percentage of material of diameter less than x, D is the diameter of the largest size fraction present, and m is a constant with a value, between $\frac{1}{3}$ and $\frac{1}{2}$, for optimum packing density.

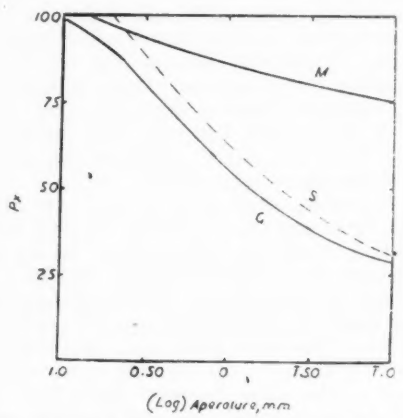


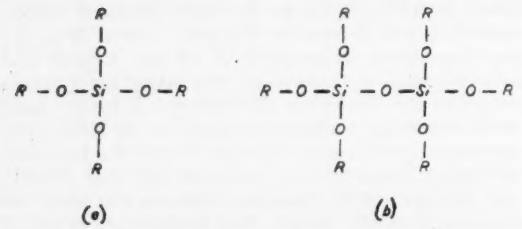
FIG. A—Curves showing size distribution in (*S*) ideal grading, (*M*) material *M*, and (*G*) material *G*

Curve *S* of Fig. A, shows the curve obtained with m equal to $\frac{1}{2}$ and D equal to 4·0 mm. Curve *M* shows the distribution in material *M* of the Report and curve *G* that of material *G*, the latter being much nearer to the ideal than the former. Actually, none of the materials we have examined so far have come as near to the ideal as material *G*, but the tendency to better properties, as measured by bulk density and strength, with variation towards the ideal has been fairly clearly shown. Size distribution is not, of course, the only factor to be considered, for though bulk density depends mainly on distribution, strength is largely influenced by actual particle size. Also, it is possible that with materials of low refractoriness, approach to the ideal grading distribution may not be as important as with more refractory mixtures.

On the question of chemical composition, one of the first constituents to which anyone approaching this problem of monolithic linings would attach significance, would be the alumina content. The effect of alumina on the resistance of silica to fluxes is well known, in that it first reduces and ultimately causes the disappearance of the range of liquid immiscibility occurring in the basic-oxide/silica systems. The high-melting-point plateaux in these systems thus disappear, low-melting eutectics are formed, and solubility of solid silica in the liquid phase is markedly increased.

There are other factors to be considered as well, however. All the monolithic lining materials we have examined so far have been of surprisingly low refractoriness, slumping occurring in test pills below 1550° C. generally. When we saw the coloured photographs of the used bricks, reproduced in Figs. 12, 13, and 14 of the Report, we were struck by the difference from what we have seen when examining used samples of such linings. The silica bricks show iron oxide penetration to a depth of something like 1½–2 in. The used monolithic linings, on the other hand, have shown only very restricted slag penetration, there being a sharp line of demarcation between the adherent slag crust and the adjacent zone of the refractory, which is highly vitrified or fused to a depth of 1–2 cm. from the hot face. Correlating this with the test behaviour of the refractory, it seems clear that with these monolithic materials of low refractoriness, we get progressive vitrification, amounting almost to fusion, before the flux has penetrated, the temperature in the neighbourhood of the hot face being sufficient to do this. The fused layer so produced, presents a more or less continuous barrier, free from capillaries, to the penetration of the flux, and the penetration that does occur is more in the nature of a slow diffusion of oxides, or possibly of ions, into the mass. With the more refractory brick linings, flow of the fluxes along capillaries has almost certainly been involved.

The question arises as to how such materials stand up at all in use. It seems to be mainly a matter of the very high viscosity of the fused zone or layer formed at the hot face. Various speakers have referred to the probable importance of the steep temperature gradient through the refractory, arising from the intermittent nature of the process, and we should agree with this in that it will limit the thickness



(*a*) (*b*)

FIG. B—Structure of vitreous silica corresponding to (*a*) the orthosilicate and (*b*) the sesquisilicate

of the fused layer. The reason for the high viscosity of the latter can be understood from a consideration of the constitution of silica glass. Vitreous silica consists of a continuous network of silicon–oxygen tetrahedra linked through their corners by sharing common oxygen atoms. The network formed is three-dimensional and extends throughout the whole mass of the material, very much like the state of affairs found in an organic ' plastic ' after polymerization. As basic oxides are added, the network is broken down to an extent depending on the basic oxide content. Thus, when the composition corresponds to that of the orthosilicate, the structure has broken down to individual tetrahedra as indicated in two dimensions in Fig. B (*a*) for the case of the oxide of a monovalent metal, R_2O. At the sesquisilicate composition the tetrahedral units are linked in pairs, as shown in Fig. B (*b*), while at the metasilicate composition six-membered rings of tetrahedra are probably formed, and so on. Thus, as the silica content increases, the size of the unit particle increases and, as a direct consequence, the viscosity will increase.

One point that seems to emerge is that with materials of low refractoriness, the highest possible viscosity above the fusion point is desirable. If this type of lining material is found to be the best in service it might be worth while trying out the effect of additions to relatively pure silica rock, of materials which would lower the fusion point while causing a minimum degree of break-down of the silica network. In other words, if we made linings with relatively pure materials, and added substances known to be network-formers (boric oxide is one, but others more suitable might be found), we might get improved durability, though whether this would be commercially feasible is another matter.

The last point I should like to refer to is in connection with the mineralogical constitution of the slags and the zones of the used bricks. I think that it should be pointed out that the reported constitutions are, in the main, in agreement with those we should expect from the phase-equilibrium relationships of the system involved, allowing for the formation of glasses, of course. I do not, however, think that the relationship shown to exist between silica content and constitution, in the case of the slags, has any fundamental basis. With the silica contents indicated, the nature of the silicate should be determined by the ratio of the metasilicate-forming bases, such as CaO and MnO, to the FeO, after allowing for the required FeO to form spinel (magnetite). With

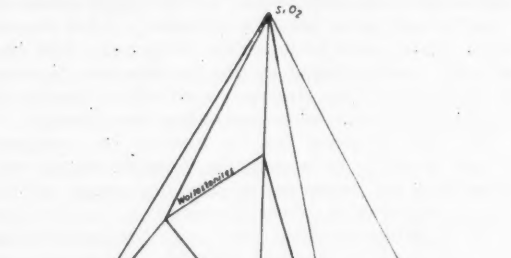


FIG. C—Quaternary system $CaO–FeO–Fe_2O_3–SiO_2$

high values of this ratio, the chief constituents would be silica, wollastonite solid solutions, and spinel, and with low values silica, fayalite, FeO-saturated wollastonite, and spinel. The relationships involved are illustrated in Fig. C for the quaternary system $CaO–FeO–Fe_2O_3–SiO_2$. This does not allow for the effect of MnO, but as a metasilicate-former the latter can be grouped along with CaO. Hedenbergite results simply from the breakdown of wollastonite solid solutions on cooling, and its formation would probably depend on the rate of cooling and the composition of the original wollastonite. The apparent significance of the silica content appears to be due mainly to the fact that in the slags examined, the ratio of CaO plus MnO to FeO tends to fall with decreasing silica.

Dr. **G. R. Rigby** (British Refractories Research Association); My main interest in this Report has been in the petrological work to which Dr. Cross has alluded. The most interesting feature emerging from the petrological examination was the close similarity between the used bricks from the converter, and similar bricks from open-hearth steel furnace roofs. Dr. Cross mentioned that in the case of the open-hearth roofs, used bricks show a series of well-defined zones. There is a cristobalite zone at the hot face and behind this a tridymite zone. This is in accord with theory, since cristobalite is the stable phase of silica above 1470° C., and tridymite the stable phase between 1470° and 870° C., while quartz, of course, is the stable phase below 870° C.

The interesting point with the converter bricks is that all these zones are squeezed into the first few millimetres behind the working face. At the face of the brick there is a cristobalite zone, but this extends only for a few millimetres, whereas in open-hearth furnace bricks the cristobalite zone can be measured in inches. This must mean that the temperature gradient through the brick is very much steeper in the case of the silica converter lining than in the case of the open-hearth roof; and that the temperature drops below 1470° C. at a position a few millimetres behind the working face. This has a very

important bearing on the rate of wearing back of the lining. The petrological work is, therefore, in agreement with the actual temperature measurements which have been made through the converter linings, which also demonstrates that there is a very steep temperature gradient from the surface into the brickwork.

There are one or two curious features about the cristobalite phase in silica bricks. Although cristobalite is the stable phase of silica only above 1470° C., nevertheless this form of silica appears in unused silica bricks, which have never been heated to a temperature as high as 1470° C. Under the microscope, the cristobalite which has been formed as a metastable phase, by the conversion of quartz at a temperature below 1470° C., presents different features from the cristobalite which has been formed at temperatures over 1470° C., particularly with regard to crystal size, and it is possible to differentiate between them in a thin section. The cristobalite present at the face of converter bricks has similar characteristics to the cristobalite which is present in the used open-hearth bricks : that is to say, it has been formed at temperatures similar to those prevailing in the open-hearth furnace. Behind the cristobalite zone is a tridymite zone, in which the cristobalite present in the unused brick (which has been formed below 1470° C.), as well as any residual quartz, are both converting to tridymite. This conversion is helped by the presence of ferrous oxide, which catalyses the formation of tridymite. From the above remarks, it will be seen, therefore, that the petrological features and the X-ray features also, throw light on the temperature gradients in the converter lining, and are in agreement with the actual measured temperatures. The steep temperature gradient which occurs through these converter linings is undoubtedly an important factor in reducing the rate at which the lining erodes back.

Dr. **W. A. Archibald** (B.I.S.R.A.) : Dealing with the Report generally, I would say that it lists a very large bulk of exceedingly useful material and data which is not, I suggest, made use of to the utmost possible extent. There are a few questions which I should like to ask in order to see whether this information is going to be, at a later date, as useful as it might be.

There are one or two things which do not quite fit in. First, in Table I (*b*), Plant *J*, we have a figure of 127 heats per run. I doubt whether this is correct ; it does not agree with the average figures given later. Secondly, in Table II (*a*), Plant *N*, there is a figure of 249 lb. refractories consumption per ton of steel, and none for patching. If that is correct, the average figure given in Table III (for 40–47-cwt. plants) are wrong, as also are the ranges. For example, the refractories consumption for the lining, in lb. per ton of steel, in Table III, should be 4¼ to 249 instead of 4¼ to 115, and the patching, in pounds/ton of steel, should be zero to 135 instead of 5 to 135. The importance of this is that if these figures are taken, the general conclusion with regard to the consumption of refractories is not true. The figure for the smallest vessel capacity (23–30 cwt.), which is 105 lb., still remains correct, but for the 40–47 cwt. range it must go up to 144 per lb. ton of steel, leaving the figure for the largest plants (50–60 cwt.), at 48 lb. per ton of steel. This seems to me to be important, as it would mean that plants of medium size use the greatest amount of refractory material per ton of steel.

On page 249, of the Report, it is mentioned that there seems to be a relationship between the clay content and the permanent linear change. There are only four results which can be used in this connection and, after plotting them, it seems to me that it is not possible to put any emphasis on that statement. There are one or two other statements which seem to me to fall into this category of being backed by insufficient evidence, at least as far as the data available in the Report are concerned, and which I should like to hear vindicated if possible. I do not see any adequate evidence, for instance, for the statement that over 90% of silica appears to be desirable. It seems desirable that it should be high, but there do not seem to be any results included in the Report to support the statement that it should be over 90%.

With regard to the remarks on preheating, it seems to me that some kind of experiment should be done to ascertain the maximum safe rate. I would suggest employing a dummy converter, built dry with silica bricks, or at least jointed in such a way that the bricks could be subsequently removed for crushing-strength determinations. This should show what rates produce a serious loss of strength.

On page 257, the Report deals with diminishing the the time interval between blows, and the statement is made that the average temperature at the hot face is some 90° C. higher with sequential blowing than with alternate blowing. That seems to me to be somewhat questionable. The temperature along the peaks is rising all the time. It seems to me that the distance of the couple behind the hot face is diminishing, and the temperature is rising in any case. That need not be linear, and the slight increase there could still be due to that cause.

These are just a few points which occurred to me on going through the Report, and some explanation of whether they are justified or not would make the Report still more valuable as a collection of information, from which many more conclusions could be drawn at a later date.

The **Chairman** : We are indebted to Professor Sarjant and to the members of his Sub-Committee for the way in which they have presented the Report, and to those who have taken part in the discussion.

The meeting then adjourned until 2.15 p.m.

PROCEEDINGS OF THE AFTERNOON SESSION: 2.15 to 5.0 P.M.

Discussion of the FIRST REPORT OF THE SIDE-BLOWN CONVERTER PRACTICE SUB-COMMITTEE,*

of the Steel Castings Research Committee

Dr. **T. P. Colclough** (Chairman of the Sub-Committee), in presenting the Report, said: The first thing that I have to say in presenting the Report is that its title is not quite complete, because it should read "First and Last Report of the Side-Blown Converter Practice Sub-Committee." This Sub-Committee has now ceased to function, and is merged in the Melting and Metallurgical Committee of the British Iron and Steel Research Association. I should like, therefore, to give a sort of farewell blessing to the new body which is taking over the work, and to wish them god-speed and far greater success than we were able to attain.

This work had its origin in the efforts of Mr. Paul Fassotte during the war, to stimulate output and improve working conditions. At the end of the Report there are acknowledgments to the various firms and individuals who have contributed to the work, and I should like, as one outside the industry, to express appreciation of the tremendous amount of work put in by the members of the Sub-Committee. The success achieved has been really remarkable. I would particularly mention the work of Mr. Spray, the Secretary of the Sub-Committee, and I am sure that every member of the Sub-Committee would wish to join in expressing our gratitude to him.

DISCUSSION

Lieut.-Col. T. B. Biernacki (Polish Technical Institute): I am very pleased to have this opportunity to open the discussion on this remarkable Report. I wish to say a few words about it, because as far as I am aware it is the first serious and successful attempt to widen our knowledge, which is very limited indeed, about the side-blown converter, and also because one of the members of the Sub-Committee was my close friend, with whom I worked for many years in Poland and in this country.

The converter process has been neglected for some time by metallurgists all over the world, because they found open-hearth and electric furnaces to be much better tools for making steel; but during the war, in the U.S.A. and to some extent in Great Britain, the side-blown converter production of steel castings has shown an upward trend. In the U.S.A. for example, in 1933, 12 steel foundries were using the side-blown converter process, employing 17 converters; while in January, 1944, 37 foundries were operating the process, with 66 converters.

The main factors contributing to the revived use of the side-blown converter, apart from the high demand for steel in war-time, are the improved and successful methods of desulphurization and dephosphorization of cupola metal, the development of a triplexing method using the cupola–converter–electric-furnace combination, which permits a continuous supply of molten metal to the moulds, and improved methods of control of the converter blow. The lower installation costs of the side-blown or Tropenas converter plant, in comparison with any other steel-making unit of equal capacity, were also responsible for this revival. Finally, the flexibility of the Tropenas converter in the production of carbon and low-alloy steels was one of the major features of interest.

Turning to the nature of the process, the main conclusion of the Report is that the side-blown-converter process appears to be predominantly one of metal–slag reaction, and not one of metal–air reaction, as in the bottom-blown Bessemer converters. Consequently, there should be much more similarity between the side-blown-converter process and the acid open-hearth process than between the side-blown converter process and the Bessemer process.

The Report refers to many similarities which confirm this conclusion, and I shall mention two of them. First, in spite of the wide variations in converter design and operation, and in the composition and temperature of the metal charged, at the four plants under investigation the quality of the finished steel was uniformly high, having properties similar to those of open-hearth steel. Secondly, the silica in the slag was reduced in the last stages of the blow, this reaction being accelerated in the case of high-temperature heats.

I shall not question these similarities, but from the data given in this Report itself, and also from what we heard this morning about the wear of the linings of the side-blown converter, there are good reasons for believing that appreciable similarity also exists between the side-blown converter and the bottom-blown converter processes. For example, it is common in the acid Bessemer process, for the oxidizing action of the iron oxide to be first directed towards silicon and manganese; oxidation of the carbon becoming lively only at the end of the blowing. The same thing happens in the side-blown converter heats, as can be seen from Fig. 2 and Table IV, the heats at Plants *A* and *D*. In both cases 85% of the silicon was removed before the carbon elimination proceeded rapidly.

Secondly, it has been found in the Bessemer process that hot pig iron of low silicon content facilitates a more rapid removal of carbon than a cold iron of higher silicon content. This also happened in experimental blows at Plants *A* and *D*. At Plant *A*, high silicon cupola metal at about 1300° C. was charged to the vessel, with the result that the removal of

* *Journal of the Iron and Steel Institute*, 1947, vol. 155, Jan., pp. 33–50.

carbon began somewhere after 17 min. of blow, when the silicon in the metal dropped from 1·2 to 0·7%, or perhaps lower. At Plant *D*, low silicon metal at higher temperature, namely about 1450° C., was charged to the converter, with the result that the oxidation of the carbon commenced almost immediately at the beginning of the blow. An interesting point emerging here is how the oxidation of the carbon would proceed if hot metal with a high silicon content were introduced into the converter at the established critical temperature of 1450° C., when the oxidation of carbon becomes rapid.

Thirdly, in the Bessemer process it can be assumed that the equilibrium between steel and slag as regards the partition of iron oxide, does not exist, because the rate of transfer of the iron oxide into the slag is lower than its rate of supply to the steel. Does this equilibrium exist in the side-blown converter ?

It is not possible to reply to this question without the figures of the iron oxide content, during the blow, in the steel and in the slag. Unfortunately, the investigations include only the FeO content of the slag. For the two blows at Plants *A* and *D*, Table IV, it is interesting to note that in Plant *A*, with the metal of high silicon content, during the removal of silicon, FeO in the slag was slightly increased, from 53·8 to 54·5%, and a marked decrease was observed during the removal of carbon. At Plant *D*, with the metal of low silicon content, when the removal of carbon began immediately after 4 min. of the blow, the FeO content in the slag remained almost unchanged, and in the following 2 min. of the blow, during which the oxidation of carbon in the steel proceeded rapidly from 2·6 to 2·0%, the FeO content in the slag was not decreased, but increased very substantially, from 29·4 to 39·3%.

Incidentally, very similar results were obtained by Fassotte when the converter charge composition was 2·7% of carbon, 0·2% of silicon, and 0·35% of manganese. The analysis of FeO in the slag was as shown in the second column below, the figures in the third column relating to Plant *D*:

Duration of Blow, min.		FeO in Slag (after Fassotte), %	Fe in Slag (at Plant *D*), %
2	...	23·2	29·9
4	...	25·7	29·4
6	...	25·1	39·3
8	...	25·2	28·8
14	...	22·5	25·5

From these figures it seems that only two conclusions can be drawn. Either there was one metal–air reaction in the bath, or the necessary oxygen was not supplied from FeO from the slag but from FeO formed in the metal itself. However, when the air is blown into the vessel under the pressure of 3 to 5 lb./sq. in. at a blowing angle varying from 8° to 20°, and as we were told this morning, even to 63°, undoubtedly some air will be mixed with the metal, resulting in the formation of FeO, not only on the surface of the bath but also in the steel itself. Even when the blowing angle of the air will guarantee that the air is blown only on the surface of the metal, some mixing of the air with the metal will be inevitable, owing to the agitation of the bath during the blow.

It seems to me, therefore, that we may possibly find ourselves on the correct road if we assume that the process is that slag–metal and metal–air reactions are taking place simultaneously. The main reaction especially at the end of the blow may be, of course, between the slag and the metal, but this largely depends on the blowing angle of the air to the vessel. This point may be of paramount importance to the character and nature of the side-blown converter process, and I think that it would repay investigation.

Finally, I should like to point out that the presence of free carbon in the gas may be due not only to the reduction of carbon dioxide or monoxide, but also to the formation of graphite which, displaced by the silicon from the metallic solution, remains in the form of suspensions in the metal, or partly collects at the surface of the bath. This graphite, during the agitation of the bath, might go into the gas.

I should like to congratulate the Chairman and all the members of the Sub-Committee on this Report. The very short time in which this process can take place makes its control and also any experiments very difficult. They have not had an easy task to perform.

Mr. **P. C. Fassotte** (Leeds) : The members of this Sub-Committee were good enough occasionally to ask me to be present at their deliberations, and I am sure, therefore, that it will not come amiss if I start on a gentle note of criticism, as I am prepared to take my share of the blame. The criticism concerns only the point of presentation. Members of this Sub-Committee had unquestioned authority to express opinions on the subject ; they have done an astounding amount of work, and when they reached the stage of conclusions, I think they were entitled to be more forceful and to present some of them in a more emphatic manner.

I refer, particularly, to what is unquestionably one of the most important points which have been brought to the fore by this Report ; namely, the attention drawn to the importance of the total silicon content of the metal to be blown. The very economics of the side-blown converter process hinges on this question of total silicon content of the metal, irrespective of the time when the silicon is added and the form in which it is introduced. We must bear in mind that the silicon in the converter is only acting as a fuel. It is a fuel, however, which in whatever shape we put it in, is costing us say, £50 per ton, and the steel industry is subject to a chronic fuel crisis.

There is not only the question of cost, however, to be borne in mind. In some very interesting graphs (Figs. 12, 13, 14, and 15) the Sub-Committee draw attention to the effect of variations in silicon content. It takes the reader, however, some effort to discover the conclusion, and it would have been helpful if the Committee had pointed out that whatever plant the user is employing he should watch his total silicon content, and work with the minimum which is compatible with his plant and his object. The members of the Sub-Committee have produced the evidence. The graphs illustrate the overwhelming importance of the total silicon content on the blowing time, on the mechanical loss, on the total loss, and also the influence of initial temperature on mechanical

loss, which is largely influenced by the silicon content. The use of low silicon iron promotes not only reduced losses, cost, and also increased life of refractories, but adds considerably to the flexibility.

I shall not take up time by examining any of the details of the Report. I have no quarrel with them. The implications of this Report, however, are more far-reaching than appears at first sight. Those who are building the steel foundries of tomorrow will be bound to take cognizance of this Report and to weigh up carefully its findings. The general tendency of all foundries, whatever the metal, is in the direction of increased mechanization, and increased mechanization perforce goes hand in hand with the more constant availability of hot metal. Thanks to the work which has been put in by this Sub-Committee, it is now possible to build steel-melting plants which, without the addition of holding furnaces, will produce any desired quantity of metal, say, from 6 to 20 tons/hr., with intervals between heats which may be in some cases as low as 5 min. only, and creating, therefore, the very conditions which the foundry engineer requires to feed with metal his mechanized system.

I was disappointed to hear Dr. Colclough say that this is the first and also the last Report of the Sub-Committee. A great deal of further work is still required. The average losses incurred, as revealed by the Report, are subject in my opinion to an important further reduction. The influence of the internal shape of the converters, and of pressure and volume control needs further investigation. There is also the possibility of enriching the blast by an introduction of oxygen, and I am glad to hear that this subject is already beginning to receive attention. There is also the question of the extent to which the converter process lends itself to the production of low-alloy steels. Colonel Biernacki made an allusion, presumably to our friend Mr. Jazwinski. I well remember the surprise of Mr. Jazwinski, after having been for the first time in charge of a converter plant, at the excellence of the steels which he was able to produce by this process. Equally many metallurgists, and I would even go so far as to say some users of converter plants, would be surprised if the full extent to which a converter can be used for the production of low-alloy steel were revealed to them.

In conclusion, I would like to say how very much I personally have appreciated this Report. I think the members of the Sub-Committee can be proud of their work. They have produced, what is in my opinion, the most authoritative and trustworthy piece of work in the whole of the literature on the side-blown converter.

Mr. **J. H. Cooper** (Robson Refractories, Ltd.) : I should like to approach this matter from another angle. We want to improve converter practice, and I contend that the converter has been in disgrace for a long time on account of carelessness in working. A great deal of that has been removed by the excellent Report now before us, which we must all admit is one of the best that we could have had.

The question of saving the linings has been raised, and the heating of the linings suggested. It is my contention that the converter should be heated by oil, in exactly the way in which the Stock converter is used today. There is no mention of the Stock converter here, but it was originally made as an improvement on the Tropenas, to melt the metal in a converter and be blown afterwards. It served a very good purpose, because we got no sulphur pick-up and we had good blowing. That regeneration could, of course, be applied to the Tropenas plant very easily, which would again save time in blowing.

Silicon can be used in large or small additions, according to the quantity of metal which you charge, but I think there will be general agreement that this can be cut too fine. Another question is the addition of sand to save the lining. My contention is that the addition of sand is best made after the converter has got the flame, because then you do not get it scattered, but added to the slag which is boiling, because when the vessel comes on the boil in the first place, you get the converter metal and the slag together. You have proof of that, because sometimes it just runs over at the mouth.

Care must be taken about the use of the blast. I have seen scores of converter plants in this country and elsewhere, where the system is to put the blast on, and there it stops until the blow finished. But I do not agree with that at all ; I think that the blast should be put on full at the initial stages and afterwards reduced, because it is my contention that it is not the amount of air which you put in the converter which is beneficial, but the amount which is put in the converter and used. You supply the air to support combustion. I had the advantage of working for years on experiments to find out what was the best way of blowing converters, and how to economize ; some of the converters which we had to deal with, and which are in existence today, prove the truth of my contention. If you support combustion by the air supply, you do not need extra pressure at all.

Steel scrap can be used, and is always an advantage; but a little pig iron is sometimes very useful, because we have to keep before us the things that are essential, and the fluidity of the metal is what counts ; the ability to put metal in the ladle and leave it in the ladle for an hour or longer. Some of my friends here have blown converter charges three and four heats after each other, which have been cast in good jobs, and the metal has not gone cold at all. That cannot be done if you use too large a percentage of steel scrap ; it requires a certain percentage of iron in the cupola charges.

Economy sometimes costs more than it is worth. Silicon is oxidized or burnt in the bath, which generates heat in the metal. Carbon, either carbon monoxide or any other oxide, burns on the surface of the bath, and we know very well that heat which is generated on the surface of the metal is never as good as that which is generated in the bath. We have proof of that in the Siemens furnace.

Converter practice fell into disrepute many years ago, because we got cracks and trouble of all sorts, difference in carbons, and difference in all sorts of results ; but those, of course, can easily be overcome. I recommend, also, the use of the spectroscope to take note of the blowing. You should take notice of

your carbon and of your sodium line. It is really all very easy. I am not making these remarks as criticism; I am making them to be helpful, because I want to help every converter man and every steel user to take advantage of anything I may say, and I want to place my experience at your disposal. The use of soda ash has given us advantages in the reduction of sulphur, which we did not have a few years ago, and I believe that we are on the fringe of greater improvements. I am not so clear about the position so far as phosphorus is concerned.

There is another thing which we must keep before us all the time, and that is the composition of the metal. Some people are blowing charges in 7 min., others 15 min., and others in 25 and 35 min. There is no uniformity; but it is not uniformity in the blowing but in the composition of the metal which matters. I recommend every converter user, therefore, to look after the condition of his metal, to look after the depth of his bath, to see that his lining is good and that his tuyeres are kept clean, and not to be afraid of putting a little tar round the tuyeres when ramming the material in. As far as the grading of the refractory lining is concerned, there are many things that can be said for and against, but it all depends on the position and the physical properties of the silica in the converter ramming.

Mr. **J. L. Harrison** (Messrs. Catton & Co., Ltd.): It is a very great pleasure for me to be here this afternoon when this Report is presented, because I have been very closely associated with most of the work which has gone on on behalf of the Sub-Committee. I think that my closest association was during a very hot summer, when I was obliged to stand holding a bent piece of tube inside a vessel mouth, trying to obtain gas samples. I do not want to be any more closely associated than that!

Reference has been made to the very vexing question of sand additions. I am not here to reply to Mr. Cooper, but personally I should not like to try adding sand during a blow; I do not think that very much of it would arrive at the desired spot. The question of sand is mentioned both in this Report and in the one on converter refractories. It is probable, or at any rate possible, that some iron oxide reacts with the silica at the commencement, but it is largely a mechanical action. At one time I was concerned with a converter plant which had quite a thin lining of 2½ in. working face, and I am at present concerned with the problems of a converter plant having a much thicker lining. With the plant having the thinner lining we found that the silica addition did not function at all, but with the lining which we are using at the present time we find an advantage from it. The reason is that the sand builds up on the bottom. With the thinner lining, the lining wore away before any benefit from the sand addition was apparent; but with the thicker lining the sand has a better chance of building up in the well of the converter at the same time as wear is taking place higher up, and we get the benefit in that way rather than from any direct oxidation.

It is a little unfortunate that this Report deals only with the results taken from one particular heat, and that it took 50 min. to blow, which is contrary to general practice. A more normal time is 20 min. I would suggest that in any future work a large number of results should be taken, the averages worked out, and the whole matter studied more on a statistical basis rather than on the basis of individual heats. We have done some work interrupting each blow once only and averaging the results from interruption after given time intervals, finally constructing from a large number of results the curves for a typical blow. We have found that the results are much more reliable than taking individual heats.

The Report suggests that carbon is one of the main elements in increasing temperature. I do not quite agree with that point of view, because for each pound of element present more heat is produced from silicon than from carbon. It is, however, a very expensive way of producing heat, and future work should be directed towards reducing the quantity of silicon required.

The Report also seems to suggest that a temperature of 1450° C. is rather critical, and at that temperature carbon begins to oxidize. I think that that is quite true up to a point, but work which we have done since tends to indicate that it is not so much the temperature that is important, as the elimination of silicon, and that the carbon generally does not go in any great quantity until all the silicon and manganese have been eliminated. I should like to ask the Sub-Committee what their views would be if one had a bath containing 0·5% of silicon, and that was pre-heated to a temperature of say, 1540°–1550° C. What would happen? Would the silicon oxidize or would the carbon oxidize? We found, in work which we did last summer, that the carbon did not commence to oxidize appreciably until all the silicon was eliminated, and that occurred somewhere about a temperature of 1540° C.

Work has been done recently in connection with bath temperatures, and a good deal of information has been got together. It is possible, knowing the initial bath temperature and the silicon content, to predict the final bath temperature, and it is also possible, knowing the initial bath temperature, to decide how much ferrosilicon is going to be necessary to bring the bath up to the desired temperature.

My personal view is that the silicon content of the metal, under present-day conditions of temperature, is important, and that there seems to be a critical figure of something like 1·4%.

I have had the opportunity in the last few days of seeing the results of some work done in Germany. I am not sure of the origin of it, but in the experiments carried out, high silicon iron was used up to well over 3%. It is rather significant that there is almost no temperature change in the metal—they started off somewhere about 1300° C.—until the silicon dropped to about 1·4%. Speaking purely from personal experience, I have found that silicon round about that figure seems to give the best results. Moreover, one seems to get a much better temperature increment if one starts with a little less silicon, in the region of 1%, and add the remainder during the blow.

I wonder whether any members of the Sub-Com-

mittee would care to comment on that and give us the benefit of their views, as to whether they think that original silicon, or silicon added during the blow gives us the greater temperature increment, or whether there is nothing in it ? The German work to which I have referred also suggests that silicon is reduced from the silica towards the end of the blow, and also that there may be endothermic reactions going on at that time. They show the maximum temperature some minutes before the conclusion of the blow, and a slight drop in temperature towards the end.

There are one or two other interesting points which we have come across in studying converter heats. In the early part of the day's run heat is absorbed from the metal into the lining, and later on from the lining into the metal.

Going back to the question of the higher silicon, it would appear that the longer the blow takes the greater the radiation losses, and I think that sooner or later we are going to come into some slight conflict with the refractories people with regard to this phenomenon. The refractory reports generally suggest that to get the longest life out of the linings we must conduct heat rapidly away from the working face of the lining, whereas most of us are anxious to retain heat in the bath. I feel that sooner or later there will be a conflict between us there.

I wonder whether the Sub-Committee have any views on the question of high manganese charges and their effects on the blowing operation, on mechanical losses, and on the lining life ?

With regard to future work, I believe this should be directed towards evolving the ideal bath dimensions and the ideal shape, and arriving at the correct number of tuyeres and their size, and the correct blast volume. These are questions which have not been satisfactorily settled. A good deal is said in the Report about them, particularly in the Tables referring to the various plants, and there are conflicting ideas and a great many variations there which need clarifying. We ought to aim at speeding-up the blow generally for various reasons—to minimize radiation losses, to give a much higher temperature in a shorter time, and also, what is very important, to try to reduce the amount of ferrosilicon, or silicon in any form, added for converter-steel production.

Work has started in that direction, and those of us who are pursuing it are hoping to get some interesting results on oxygen enrichment for, I hope, a second report when it is issued.

I feel that if we can tackle some of these problems, and if we can pay a good deal of attention to the question of sulphur and phosphorus removal, then we can produce in the converter a steel which will compare very well with the best electric arc steel produced.

Mr. **H. O. Howson** (Blaenavon Co., Ltd.) : I would like to congratulate the Sub-Committee on getting together so much valuable information, but I think that we can offer a certain amount of criticism on the views on the fundamental reactions which they have put forward. They have accumulated much data, but in my view there is rather a lack of practical observation on the process, and I think that practical observation is still the mainstay of any scientific approach to steelmaking.

I agree that the converter process may be most conveniently divided into three stages, but in the first stage, for instance, the operations are definitely more complex than are mentioned in the Report. Usually cupola metal has been introduced into the converter some short time before blowing commences—on an average, about 10 min. before—and, as we all know if one allows cast iron or cupola metal to stay for that length of time, a film of kish or graphitic material is developed on the surface. The first action in the converter practice is the lighting up and burning of this kish formation. It does not always take place immediately, because very often a slight chilling effect of the bath occurs, but that is the fundamental reaction. Afterwards, I agree that FeO is formed, but that is a very important point in lighting up.

This leads to the point which I wish to emphasize; I can see no reason at all why carbon in the metal cannot actually burn in a stream of air in precisely the same method as a brazier full of coke burns easily in air. I cannot see why the carbon cannot burn out of the metal in a similar way. I submit that the effect of air jets in a converter is to play upon the surface of the metal and cause a very pronounced agitation. This enables the air and metal to mix intimately, and the reaction ensues. I think this is shown in the work of the Sub-Committee by the fact that when $2\frac{1}{2}$-in. tuyeres were employed at Plant *E* no reaction was observed. I submit that the jet effect must take place in converter operations to agitate the bath and mix air and metal intimately. As this mixture of air and metal takes place it becomes progressively more vigorous. I believe—and I think that Lieut.-Col. Bternacki introduced this—that in the second stage of the converter process, that is, the boiling stage, the converter action is fundamentally a reaction between air and metal. I do not think that there can be any doubt about that.

The Sub-Committee have put forward the point of view that there is a very close similarity between the side-blown converter and the acid open-hearth processes. I am inclined to agree thoroughly ; as most of my experience has been and is with the acid open-hearth process, and there is a similar reaction; but I maintain most strongly that even in the acid open-hearth process the primary reaction in the boiling period is between the metal and the furnace atmosphere. I have spent many hours looking at the acid open-hearth furnace, and was looking into one yesterday. I am convinced that when the boil commences a vigorous and intimate mixture between metal and air is obtained. I think that Dr. Whiteley showed in the very early days—in 1921, or thereabouts —that the particles of metal thrown upwards from the converter are quite high in carbon, but the metal descending through the slag is oxidized ; it has a blue oxide film, and has a lower carbon content. We have to accept that as a possible viewpoint, and that the slag–metal reaction is quite out of place in side-blown converter work.

I do not agree with Mr. Harrison that the data submitted with regard to Plant *A*, and expressed in

Fig. 2, are for average conditions, in view of the 50 min. for completion. The way in which I regard converter operations in the normal plant not charged with pre-heated metal, is that the first stage takes place for about two or three minutes, and then the boil commences. The first boil is very vigorous, in fact some converter operators refer to it as *the* boil. In that preliminary period the silicon is removed very rapidly together with the manganese but very little carbon, and then carbon removal commences. But the point I wish to bring out is that one cannot regard silicon as a kindling agent in any respect. The oxidation of carbon may take place at quite a low temperature, and the boil has to commence before silicon oxidation takes place. I am opposing the Sub-Committee in saying this, but that is my point of view.

Then we come to the third stage of the converter operation, and I do not think that the Sub-Committee have been very precise about what they refer to as the third stage. To me it is very definite, and it is what the old converter operator calls the carbon flame. It usually comes into operation between 0·5 and 0·7% of residual carbon. It is a very intense transparent flame, and is in fact distinguished by its transparency, and there is usually a feathery effect. When a carbon flame comes into operation, ejections from the converter cease. In the boiling stage, one may approach the mouth of the converter and definitely see that the air and metal are intimately mixed, and in fact, the metal is thrown up to the mouth of the converter. But when the carbon flame stage arrives the ejections cease, and the boil steadies off, and I agree with the Sub-Committee that the reactions are then fundamentally slag–metal reactions.

The Sub-Committee have brought forward the view that silicon is reduced in the latter stages of the operation, and I am in favour of that view, but I would like to ask why they have propounded the view that the source of such silicon is the slag. In my view, only 40% of the area of the bath is exposed to the slag, and about 60% is exposed to the siliceous material of the converter. The slag contains about 70% of silica, and the siliceous material of the converter contains 95%. To my mind, a more reasonable source of such silicon is the converter vessel.

The Sub-Committee introduced the view that carbon is the reductor in this reaction. I am inclined to disagree, because when the silicon reaction takes place the carbon content of the bath is often under 0·2%, which is a very small amount to reduce silicon, and I would like the Sub-Committee to consider that point.

I should also like to say a few words with regard to the heat balance of the converter. I was on Plant *C* when a great deal of this work was carried out, and we tried to take the temperatures in the converter. It is very difficult, and I am doubtful about the results. Access is difficult and there seems to be some other detracting factor. I am accustomed to estimating temperatures, and my impression of the pyrometric determination and the visual impression is that during the carbon flame period we never have a temperature increment of more than 30° C., and on occasions I have definitely seen a decreasing temperature during that period.

Mr. Harrison referred to the German work, which also shows a decrease in temperature during that period. This is tending to show that we do not get a great deal of heat from the combustion with carbon. I think that one of the reasons is, as suggested, that if carbon monoxide is formed in the bath and is burnt to carbon dioxide, the heat is generated in the body of the converter, whereas silicon actually gives its heat up to the bath. I am still of the opinion that it is extremely difficult to operate a converter plant without substantial silicon additions in the form of pig iron or ferrosilicon during the blow. I agree with Mr. Harrison that it seems preferable to add ferro-silicon at later stages, as and when it is required. In that connection, I have seen on very many occasions the last charge of the day, when the converter is thoroughly warm, and if hot metal from the cupola is charged and blowing commences, the temperature of the metal in the converter is about 1450° C., and you can make a blow and get really hot fluid metal, having only about 0·8% of silicon in the cupola metal and needing no ferrosilicon additions whatever.

I should like to say a few words with regard to the high quality of side-blown converter steel. I have been present when many thousands of converter steel test-pieces have been pulled. I have not seen every one pulled individually, but have always made a point of looking over the test-pieces in the evening. I may have been very fortunate, but I have never yet had the experience of coming across a test-piece made out of side-blown converter steel which failed from a steelmaking defect. That has been my experience. I am prepared to admit that there is always a tendency towards over-blowing in the converter—it can happen regularly—that the analysis is always difficult to hit, and that there are heat-treatment considerations ; but, neglecting those facts, I have never come across a test-piece which has definitely failed due to what I may term a steelmaking defect.

Steelmaking quality, in my view, boils down to two factors. One is the hydrogen content of the steel, and the other is deleterious inclusions. I think that the biggest value of the side-blown converter process is that we seem to get a steel which is almost free from hydrogen in all circumstances, and we also get a favourable type of inclusion distribution. I think one reason is the very high finishing temperature, and when the alloys are added the inclusions have a tendency to flux out, but the main point is the extremely vigorous boil which is developed, which tends to rid the metal of hydrogen and harmful inclusions. My experience with open-hearth steel has not been so fortunate : I have seen many hundreds of test-pieces from open-hearth steel which have had definite steelmaking defects in them. My view is that the Tropenas process provides some of the best steel we know.

The Sub-Committee put forward the view that part of this superiority of Tropenas steel is due to the fact that the silicon reduction takes place in the later stages of the blow. My own reason for this high quality is that the reduction of silicon takes place

only for a few minutes towards the end of the blow, in the carbon flame period. In the acid open-hearth process you may have the over-reduction of silica, which gives rise to deleterious steel, and in the acid electric process you have even more pronounced silica reduction, and unless one is careful, trouble may arise. Alternatively, you can make extremely good steel by the latter process ; I am not condemning it out of hand, for it is a very valuable one, but it always has a tendency to over-reduction.

One point which might have been emphasized in the Report is the great difference in operating time in the various plants. The Sub-Committee do not make much mention of that. My own view is that there is an ideal or optimum temperature for converter operation, and if one goes above or below that, the time is increased.

The Sub-Committee emphasize the difficulties which may be found in sampling converter gases, and I agree with them that it is a very difficult matter, but I am very interested in the analysis of the material in question. The Report gives a carbon content for that material, and I should like to ask whether the carbon has been mechanically separated from the material and definitely determined as free carbon, or whether there has merely been a general direct combustion determination done on the whole product. I think that that is very interesting, and that the point should have been considered. If it has been analysed by direct carbon estimation, there is a possibility that all that happens is that small particles of steel with a reasonable carbon content have been carried over and possibly oxidized to some degree. I should like the Sub-Committee to go into that point.

I hope that this converter work will be carried on, because I have a very high opinion of the quality of converter steel. In view of its great flexibility, and of the high temperatures which can be developed, I think that it is an ideal method for some types of foundry operation.

Dr. **W. C. Newell** (B.I.S.R.A.) : I have no doubt whatever that first-quality steel can be and is made by the side-blown converter process, but there has been considerable prejudice against the process, based to some extent on the somewhat justified allegation that there is lack of sufficient control of the process. One of the points which has been raised against converter steel is the gas content of the steel produced by this process, and it would have improved the value of this Report if actual oxygen, hydrogen, and nitrogen figures had been given of the samples which were analysed. Reference is made to the figures in a general way, but it would have been an added help had the precise figures been given. It would have made it quite clear that, for example, nitrogen was not abnormal, and would have provided added evidence that the reaction was taking place *via* the slag ; because if the nitrogen is not increased during the blow, in contradistinction to the bottom-blown process, there is *prima facie* good evidence that the reaction takes place *via* something else, and it seems obviously to be the slag.

Dr. **T. P. Colclough** : I think I am right in saying that those figures have already been published by Mr. Fassotte in his paper, so that there was no point in repeating them.

Dr. **W. C. Newell** : The insertion of those figures would have improved the value of this Report, in my opinion, particularly as a reference to the figures is considered relevant on page 38 of the Report.

Dr. **T. P. Colclough** : We thought that it was already well known.

Dr. **W. C. Newell** : Another point which I should like to raise is the question of the carbon found in the gas evolved. I have only just noticed it, but I think that there is an error in the data at the end, in Appendix I, headed " Reactions and Heat Values of the Side-Blown Converter Process." It is a mathematical error, but it is quite a significant one. I refer to reaction 12, " $2CO \rightarrow CO_2 + C - 41{,}080$ g. cal. " I speak subject to correction, but if you take the difference between reactions 5 and 6, which are the basic reactions, you get a positive evolution. The whole point is that it influences the temperature effect and the reactions which we discussed yesterday ; and it is significant in this respect, that if the reaction is exothermic it is reversed by raising the temperature, and the fact that carbon is found in the gas merely means that the gas in the sampling tube is at a lower temperature. Apart from this, however, there is, of course, the possibility that carbon may be carried over in suspension mechanically.

Thirdly, Mr. Harrison raised a point which I think would receive an answer from the data in the Appendix. He said, " Supposing the bath had been at a higher temperature with a high silicon content, and was then blown." From the results given in the Report, and to be deduced theoretically from the data in the Appendix, one would anticipate that at the higher temperature you would not get the effect which you normally have at lower temperature, of the silicon going very much more rapidly, but that relatively the carbon would be oxidized out more rapidly than the silicon. I would expect, therefore, that starting with a high temperature and high silicon bath, and then oxidizing, you would finish up with a normal if not a lower carbon content, but the silicon would be higher.

Dr. **T. P. Colclough** : That is so. It was proved twenty years ago.

Dr. **W. C. Newell** : That answers Mr. Harrison's point, and I think that it is fairly well demonstrated from the Report.

Dr. **T. P. Colclough** : It was common practice on the north-east coast 25 years ago. If you start with high silicon and high-temperature metal, you finish with high silicon in the metal and you ruin the steel.

Dr. **W. C. Newell** : The crux of the matter is when and how the silicon is added, and I disagree with the remarks of Mr. Howson on this matter. As a fuel material, carbon would seem, both theoretically and in practice, to be quite suitable for raising the bath temperature provided that the initial temperature is adequate.

Dr. **T. P. Colclough** : I should like to take up a few of the points which have been raised in the discussion. The question of what would happen if metal high in silicon was blown, is one on which there is very long experience. It is well known that the Bessemer process

got into bad odour in this country—I am not referring to side-blown converter practice, but to bottom-blown converter practice—due to the irregularity of the quality of the product. The real source of the trouble was that the Cleveland ores which were used for Bessemer steelmaking became very siliceous, low in iron and high in sulphur, and they could not make an iron of low sulphur and suitable silicon contents. As a result, they got very high silicon irons, and the point which has been raised was shown in practice. If they had, say, a 4%-silicon iron, they finished up with 0·2–0·4% of silicon in the steel, and for structural steel that steel was brittle and absolutely useless. The curve in Fig. 5 was drawn specifically to show what would happen. There are three curves, the silicon loss, the rate of elimination of silicon, and the rate of elimination of carbon, at different temperatures : 1450° C. is the critical temperature at which the carbon oxidation begins. It is stated in the text that above this temperature the rate of silicon falls and that the rate of oxidation of carbon is accelerated ; it does not say that the carbon is oxidized more quickly than the silicon. If you examine that graph you will see that anywhere between 1450° and 1600° C. the silicon is still falling and the carbon is going slowly. It is only when you get to about 1600° C. that the carbon falls very rapidly.

If, therefore, you put into your converter, metal at 1450° C. with 1% of silicon and with any unspecified carbon content, and start blowing, the silicon will still oxidize first, but it will not oxidize as fast as if it were being blown at a lower temperature, and you will finish with more silicon in your metal than you would have under normal conditions with cupola metal at, say, 1300° C.

I should like to refer next to Lieut.-Col. Biernacki's remarks. It is one of the faults of English people that they are not precise in their language. The Sub-Committee were trying to emphasize the difference between a bottom-blown converter and a side-blown converter. In a bottom-blown converter the whole of the reaction must be between the air and the metal. You form a slag in the bottom-blown converter, and there is some reaction between the slag and the metal, but it may be that 90% of the reaction is air–metal and 10% is slag–metal. Conversely, in the side-blown converter the first reaction is obviously between air and metal, because there is no slag there ; if it was not an air–metal reaction it would never start at all. The first slag that is formed is always pure iron oxide. Our friends Mr. Fassotte and Mr. Spray know this full well, because in the enterprise in which we were jointly concerned, Mr. Fassotte and I were foolish enough to predict that we would get 50 blows out of the converter lining before it was due for repair. When we came on to the low silicon high-temperature metal, I think I am right in saying that the converter lining failed at about 25 heats.

We therefore observed a converter which was in operation. What happens when you turn up the converter and the air goes on? The first thing that one sees is not a carbon boil but dense red fumes coming out of the mouth of the converter—iron oxide. It was that iron oxide which was causing the trouble in the linings. As soon as that liquid iron oxide (which is one of the most corrosive agents it is possible to have for silica) was formed it started to eat out the lining, and that was the cause of the linings failing.

Sand was put in because, according to the phrase used at the time, " There is nothing more hungry than liquid ferrous oxide ; feed the brute ! " It is hungry for silica, and the cheapest form in which it can be given silica is not by ganister, but by common sand. We put sand in to start with, and the length of life of the linings went up to a very high figure.

Mr. **P. C. Fassotte** : I think it was ultimately 300.

Dr. **T. P. Colclough** : Once the slag is formed, the majority of the action is between the slag and the metal. When we have the vigorous carbon boil, Lieut.-Col. Biernacki asked why the iron in the slag rises. We have been told that the globules of iron are thrown up in a vigorous boil. That iron, as it is in the body of the converter, is oxidized and most of it falls back, but unfortunately in some cases 7% comes out of the mouth of the converter, and it is that which causes the iron losses to rise.

Lieut.-Col. **T. B. Biernacki** : I understand that, but the reaction is not predominantly a slag–metal reaction.

Dr. **T. P. Colclough** : Why does the character of the flame change after the vigorous boil ? When you have the vigorous boil in a side-blown converter, I would characterize that carbon flame as being a solid flame ; you cannot see through it because of the solid particles that there are in it. There are solid particles of iron and, I imagine, particles of manganese oxide and other oxides, because it is not possible to strike a manganese balance either. That is where the Sub-Committee failed. We cannot make a materials balance, and we cannot make a thermal balance, because we do not know how much is ejected ; and we do not know how much is ejected because of one fundamental difficulty. We can weigh the metal and everything else that is put in except air. One of the fundamental failures of our converter construction today is that we cannot measure the air going in. We can attempt to measure it by the revolutions of a pump or blower some way away, but we do not know what the losses are or what volume of air goes through the tuyeres. An experimental converter must be built with all the instruments we can think of put on to it, so that we can take the advice of the old French philosopher, " Measure, count, calculate, and then you will know what you are doing."

Then it is said that the flame becomes transparent. At this stage, the gas burning is carbon monoxide, which gives a transparent flame. The only thing that prevents it being transparent is that it contains solids, and when the carbon boil diminishes, the amount of ejections falls, and so the flame becomes transparent.

It has been noted that the temperature falls off towards the end of the blow. It certainly does, and the reason for this is given in the Report. How much excess air is being blown into that converter? If you did not have all this excess air going in, how much less silicon would you want and how much less heat would you lose ?

Mr. **N. F. Dufty** (K. & L. Steelfounders and Engineers,

Ltd.) : I agree with Lieut.-Col. Biernacki that the reactions in the converter are partly due to the gas–metal reaction and partly to the slag–metal reaction. In support of this, I should like you to consider the conditions in the converter towards the end of the carbon boil. As will be seen from the report on converter refractories, the temperature in these cases is often in excess of 1700° C., and the silica content of the slag is in some cases as high as 80%, of which at least 60% can be considered as effective. Those who work the acid electric and acid open-hearth processes will have some idea of what would happen if they had a bath of metal, in either of them, with 65% of silica in the slag, 0·1% of carbon, and at 1700° C. I think they will agree that it would very probably go off the boil, and that there would be excessive reduction of the silica of the hearth. I agree with Mr. Howson that the silica probably comes from the hearth and not from the slag.

In that case there would be an alteration in the iron–carbon–oxygen or iron–carbon–silicon–oxygen diagram, and the bath would cease to support the carbon–oxygen reaction at the air–metal interface ; in other words, the bath would go off the boil, and the boil would be merely at the slag–metal interface, if there was any at all ; that is characterized by its very gentle nature, being almost a simmer. That is the main reason why we do not produce very low carbon steels in the acid open-hearth process. If there was no reaction between the metal and the air blast in the Tropenas, you would never get down to those low carbon contents. I submit that the slag is removed from the metal surface and the metal is exposed to the air, in which case the metal oxidizes and a fresh supply of FeO flows downwards and outwards to neutralize the silicon reduction, and the carbon monoxide created burns in the blast of air as it comes from the tuyeres.

Mr. **G. T. Hampton** (Messrs. F. H. Lloyd & Co., Ltd.) : I should like to praise the Sub-Committee for the hard work which they have put into this Report, and to make one or two observations. The first is that when we consider the two Reports presented, one this morning and the other this afternoon, we see that the influence of the shape of the vessel is very important. I think that there must be some optimum shape of the vessel which will give the best refractory life, and probably also the best conditions for steelmaking.

In this Report, as far as I can see, there is no reference to the ultimate quality of the steel produced, as to the mechanical properties as well as to the general behaviour of the steel in the foundry. I should like to ask the members of the Sub-Committee whether they can make some observations on that point.

A question which has been raised both this morning and this afternoon is that of sand additions. Dr. Colclough has given a very good answer to this question and has quoted actual results. In connection with the work of the Refractories Sub-Committee, we carried out a considerable number of careful observations on heats with and without the addition of sand over a period of about two years, and the conclusion to which we came was that sand played a very important part in refractory life. In our own case, we raised the converter life figure from a matter of 60 to 80 heats without the use of sand, up to a maximum of 230 heats when we did use it ; so that from the point of view of the user of a monolithically lined converter, we can support the evidence which has been put forward by the user of a brick-lined converter on the use of sand and the improvement thereby effected in the general behaviour of the refractories.

On the question of blowing angles, we have found from our own work that blowing angles have a considerable influence on the life of the lining. In the early days of the Refractories Sub-Committee we were using excessive blowing angles, and we took steps to reduce them, with immediate benefits in lining life. We took the view that if we got improvements in refractory life by reducing the blowing angles near to the vertical, we should also improve the quality of the steel, because we should be using the air more efficiently. We therefore set out to try to carry on the whole of the blow at a single blowing position, and we found that that was entirely practical, and did not result in any disadvantage in the quality of the steel or any prolonging of the blowing process. I do put forward, therefore, for the consideration of the Sub-Committee, the investigation of a single blowing angle. It simplifies the process, it makes it more easily controlled, and it does not seem to have any disadvantages.

Dr. Colclough dealt with the points raised by Mr. Howson, but he (Mr. Howson) mentioned an interval of 10 min. between blows. I submit that that is a very uneconomical method of working, because extra silicon has to be used in order to regain the heat that is lost to the vessel by cooling in between the blows.

On the matter of blast volume, and the question of whether the entire process is carried on by means of a slag–metal reaction, there is the possibility that in the later stages of the blow it is more likely to be a direct atmospheric oxygen and metal reaction. Some time ago we carried out experiments using a very much reduced volume of blast, and found that under those conditions we got a series of false end-points of the blow. A false drop of the flame was followed by turning the converter down and rabbling the bath. On resumption of blowing the flame would reappear, persist for a short time and then drop. Although the cycle was repeated several times it was impossible to reduce the carbon to the level present at the end of a normal blow. This apparently came about either from an insufficient churning action on the bath, which oxidized the carbon out in layers, or because the rate of oxygen removal from the slag was higher than the rate at which the oxygen was put back by the blast. Would the Sub-Committee also give us their views on that point ?

Mr. **H. O. Howson** : I was very interested in the remarks about decreasing the blast pressure towards the end of the blow. I found that if we turned the converter down towards the end of the blow there was great difficulty in getting it to light up again. I have heard of occasions, though I have never seen one, where it has been impossible to get the converter to light up again ; the furnace comes completely off the boil and the slag is quiescent and forms a layer on top, and there is no possibility of the air coming into contact with the metal. I think that there has

to be a steady boil all the time to keep the process going.

With regard to the actual mechanism of the removal of carbon by the air, both Dr. Colclough and Mr. Dufty suggested that particles of metal containing carbon are thrown up into the air blast and become oxidized to FeO, which is carried back to the bath. I agree that this takes place, but if you turn up Dr. Whiteley's work on the subject you will find that it shows that the particles of metal blowing up into the air may have quite a high carbon content, 0·7%, but the particles of metal coming down which have an oxidized appearance, have a much lower carbon content, 0·3 or 0·4%, so that there is a very considerable removal of carbon by the actual burning out of carbon.

Mr. **P. Murray** (B.I.S.R.A.) : In the Report, it is stated that metal–slag reactions are predominant in the side-blown converter process, as contrasted with the bottom-blown converter which depends mainly on metal–air reactions. Previously, we have discussed rather fully, the possibilities of dephosphorization in basic-lined cupolas, and one point, on which there was general agreement, was that a considerable amount of research work (from both the metallurgical and refractory aspects) would have to be undertaken before the process can be applied on a production basis. The importance of dephosphorization in the steel castings industry has been emphasized in these discussions, and I would like to suggest that the Side-Blown Converter Committees consider the possibilities of dephosphorization in basic-lined converters. We know that basic linings are used successfully in bottom-blown converters, giving a life of the order of 180 heats, and it appears that possibly one of the main points to be considered would be the effect of the different practice on the degree of dephosphorization. I should like to ask the Sub-Committee, therefore, their opinion on these matters.

Reference has also been made to the steep temperature gradient existing in converter linings ; work carried out at Sheffield on various siliceous materials used in converters, has revealed many points of interest. In particular, some mixtures are giving reasonable life, even though their refractoriness is 150°–200° C. below the temperature of operation. Examination of used linings of these materials has shown that this low refractoriness results in the formation of a fused layer 1–2 cm. thick. Entry of slag into the refractory by normal capillary processes is thus prevented. Further, the high viscosity of the fused layer together with the steep temperature gradient experienced, appear to be the major factors responsible for the reasonably good results obtained with these low-grade materials. It is well known that the bentonitic clays give better bonding properties than the other types of bonding clay ; further, since the alumina content is much lower than in the other clays, bentonitic clays can be expected to confer maximum bonding power with a minimum increase of alumina in the mixture. The possibility of using densely graded mixtures bonded with a minimum of bentonitic clay is now being explored.

Mr. **J. H. Cooper** : I should like to comment on Mr. Murray's remark about the blowing on basic linings in the Tropenas. The difficulty of blowing basic is in the addition of lime and in taking the sample after the blow is over, because one is dependent on the after-blow of phosphorus. You obtain your heat from a low silicon, high phosphorus iron. When you are blowing in the basic process in the Tropenas or Bessemer, you are not dependent on your carbon at all ; you have to eliminate all your carbon, and the time is taken from the after-blow. When one takes the after-blow, one has to take a sample of the metal, which has to be done by fracture, and one has to be quick ; but the metal can be quite good.

One point about blowing basic and blowing acid in the Tropenas, is that you cannot over-blow basic. We have had blows in the basic converter which we have not been able to turn down, owing to some mechanical defect in the tilting arrangement, and the blow has been kept on for 10 min. longer than it should have been. You do not eliminate all the manganese in the basic, so that you are left with some of that. The basic process in the Tropenas converter can be run very successfully, but the difficulty comes from the smoke and fumes and lime and so on. It would be all right if it did not involve so much dirt.

Mr. **P. Murray** : You mean that from an operational point of view it is feasible, but the main difficulties are fumes and smoke ?

Mr. **J. H. Cooper** : And dust from the lime. You have to have the lime to throw on at the end, for burning up the phosphorus, which is what they term the after-blow. You blow until you get the carbon lines disappearing on your spectroscope, and then the time is taken from that, and it depends on the amount of phosphorus. You like to blow if possible 1·4% to 1·5% of phosphorus in your metal ; if you do not get that, you do not get hot steel. It is quite feasible, but it is more troublesome.

Mr. **P. Murray** : There is plenty of slag dust in a foundry, and I imagine that the lime dust would not be any worse than that.

Mr. **J. H. Cooper**: I do not know about that. I happen to be working in a foundry. When I spoke of the sand being put in during the blow, that idea was discounted, because it is said that it will blow out again ; but when you get the after-blow in the converter, the lime begins to powder and burn out, and it combines with the slag, but it does not all come out. But you get fumes all over the place, and they cannot all be taken up the chimney, so that it is not very pleasant.

Some people are thinking of the idea because they have no hematite and want high phosphorus iron. High phosphorus iron for blowing demands low silicon, and with high silicon you have to add a greater addition of lime. If your silicon is high you take more lime to neutralize your P_2O_5. Your P_2O_5 can go up to 15% or thereabouts. You get a thoroughly basic slag.

Mr. **P. Murray** : It is up to the Sub-Committee to keep that point in mind when they design the converter plant.

Mr. **P. C. Fassotte** : If I judge by its reception, this Report is obviously considered a most valuable one. I would like to describe it as a magnificent start.

I think I am voicing the opinion of the meeting in saying that this start must have a tomorrow, and as there are here today some of the people who will help to shape the programme of research for tomorrow, I would plead with them not to forget the concomitance of the present work. If the meeting is in agreement with that view, perhaps they will voice it. (*Applause.*)

Dr. **T. P. Colclough** : I think it would be better if the members of the Sub-Committee would come together again so as to make a reasoned reply to the very many valuable points which have been raised, and also to emphasize the recommendation which Mr. Fassotte has just made.

I am very interested to find that the suggestion of a basic-slag side-blown converter may mean having a foundry which is even dirtier than some of those which I have seen ! It has also recalled to me that I omitted one point in my opening remarks. When I said that in Germany during the war they had built a side-blown converter of 25 tons, I think I must have failed to add that it was a basic converter and not an acid one.

Dr. Newell referred to the deposition of carbon in the gas-sampling tubes. I would remind you that the decomposition of carbon monoxide into carbon dioxide nd carbon does take place. It is a very common phenomenon, and is the cause of considerable trouble in blast furnaces. It takes place in the upper part of the stack, and is commonly known as carbon deposition. It is one of the problems of the refractory brickmaker to produce a brick which will not fall to pieces by the action of this carbon which is deposited in the pores.

Two or three speakers have mentioned the 50-min. blowing time, and this raises a point which some of us have very much at heart. It is not suggested in the Report that 50 min. is a normal blowing time. It was conditioned by the fact that in order to trace the reaction, the blowing had to be stopped five times, the vessel turned down, a sample of metal taken, the vessel turned up, and the blowing resumed. What we badly want is an equipment which will enable us to take samples of slag, metal, and gas, simultaneously with a normal blow which is finished in 11–18 min., and it is only with the facilities of a research association or some body of that character that we can hope to get that established.

We want to know the volume of air going in, and the exact pressure. We want to determine the effects of different sizes of tuyeres. We must have these simultaneous samplings. We must have some correct method of measuring the temperature of the waste gases, and a whole host of other things. We plead with the Research Association to regard this matter as one of first-rate importance, and get on with the job as quickly as possible.

On behalf of the Sub-Committee I thank you, Mr. Chairman, for the way in which we have been able to present this Report, and I thank the members present for a stimulating discussion.

Dr. **W. C. Newell** : There are a few things which I should like to say in conclusion. In the first place, I take Mr. Fassotte's remarks very much to heart, particularly as they have been supported by others, including Dr. Colclough. The future of this work is of extreme importance, and the work must be pushed ahead with all the means at our disposal. I trust that all those present will be able to assist us in furthering it. On our part, we are determined to obtain the means necessary for doing it. We must face the fact, however, that very much of it depends on team work, and on co-operation in the works with which so many of you are concerned.

Dr. Colclough referred to this as being the first and last Report of the Sub-Committee. Such indeed it is, and that must be faced ; but the new Committee which is operating is to an extent an extension of the old one, and the work which it will do will be built on the foundations of this Report. I can assure Dr. Colclough that we will build on what has been done.

With regard to the meeting today as a whole, I should like on behalf of the British Iron and Steel Research Association to thank the members of the other two bodies, the Institute of British Foundrymen and the Iron and Steel Institute, who have co-operated in making this meeting possible, and I reciprocate most heartily all that Mr. Howard Wood said this morning. We shall gladly avail ourselves of the opportunity of attending the meetings of the Institute of British Foundrymen, and we trust that they will attend our future meetings and take part in our conferences.

Before we conclude, we ought to thank Mr. Cousans for taking the chair today. Professor Andrew had agreed to do so, but owing to indisposition was unable to come. Mr. Cousans at very short notice has very ably taken the chair, and we owe him a debt of gratitude on that account. (*Applause.*) Finally, I am sure that you would wish to send a message to Professor Andrew, saying how much we regret his inability to attend, and telling him how successful the meeting has been. (*Applause.*)

The **Chairman** : I should like to thank Dr. Newell for those remarks, and all of you for the excellent discussion which we have had today. The Melting Committee of the British Iron and Steel Research Association and the Refractories Panel are having a joint meeting tomorrow, and they will give consideration to today's discussion and formulate their future programme of work.

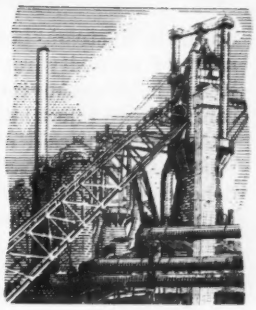
The meeting then terminated.

The Sub-Committee wish to express their gratitude to all who took part in the most stimulating discussion and are glad to find that the Report proved of interest to the industry and raised so many points on which further information is desired. In view of the wide range covered in the discussion and that many of the points raised were answered by members present, it is considered that the points remaining can well be left for investigation by the Research Association.

As previously stated, the investigations carried out had their origin in an attempt to increase production by obtaining a more complete understanding of the principles and technique involved in this process of steelmaking. The work done has revealed the gaps in existing knowledge and indicated many directions in which further work must be carried out. It will be most welcome to the industry to know that the Research Association is fully determined to put in hand a vigorous policy of investigation, under the guidance of Dr. Newell, and the new Committee can be assured of the hearty co-operation and full support of all members of the industry.

IRON AND STEELWORKS ENGINEERING

THE IRON AND STEEL ENGINEERS GROUP



THE IRON AND STEEL ENGINEERS GROUP

REPORT OF THE FOURTH MEETING

THE FOURTH MEETING OF THE IRON AND STEEL ENGINEERS GROUP of the Iron and Steel Institute was held at the Offices of the Institute, 4, Grosvenor Gardens, London, S.W.1, on Wednesday, 11th June, 1947. In the absence of Mr. W. F. CARTWRIGHT, Chairman of the Group, Dr. C. H. DESCH, F.R.S., took the Chair at the Morning Session, whilst Major W. R. BROWN (Messrs. Ashmore Benson Pease & Co., Ltd.) was Chairman at the Afternoon Session.

THE PROCEEDINGS OF THE MORNING SESSION were published on pages 541 to 562 of the August issue of the *Journal*.

THE AFTERNOON SESSION was devoted to the presentation and discussion of a paper on "Gas Turbine Applications in Iron and Steel Works," by A. T. BOWDEN, W. H. GIBSON, J. W. RAILLY, and R. G. VOYSEY.

PROCEEDINGS OF THE AFTERNOON SESSION: 2.0 P.M. TO 4.15 P.M.

Discussion on GAS TURBINE APPLICATIONS IN IRON AND STEEL WORKS*

Dr. **A. T. Bowden** (Messrs. C. A. Parsons and Co., Ltd.), who presented Part I of the paper ("Power Applications"), said: I could wish that in this paper I could deal with the subject with the same wealth of detail as Mr. Robinson gave in his paper on steam-turbine applications this morning; but in its present state of development and with the limited operating experience available, that is manifestly impossible. We have tried in this paper, however, to give a happy blend of descriptive and technical material, which we hope will meet the tastes and interests of the members.

For instance, Mr. Robinson this morning talked of the economical vacuum which one could have with a steam-turbine installation, having regard to the load factor, fuel costs, and so on; and with a long experience behind us in steam-turbine design and operation it is possible now to determine the economical vacuum to within 1 or 2 degrees. Obviously one cannot do the same with the heat exchanger, which is the counterpart of the condenser in the steam plant; but I hope that eventually we shall be able to quote with similar accuracy economical figures relating to its most economical thermal ratio.

I would emphasize that in the paper the efficiencies which we quote are not to be regarded as maxima or minima, but are to be looked upon as efficiencies which can be procured with reasonably conservative design, and they are the efficiencies which can be expected in the light of present-day knowledge, particularly of heat-resisting materials and the limitations which these put on maximum operating temperatures.

There is nothing new in using air or other gas in lieu of steam in a prime mover; in fact, most of the early conceptions of prime movers hinged about the use of air, and the early heat cycles envisaged air as the working medium. The decision to use air or steam, in spite of the advances made in compressor design and heat-resisting materials, is still a very difficult one in a large number of cases. There are advantages and disadvantages in both. So far as air is concerned, there is the drawback that we have to produce a large amount of energy to bring its pressure potential to the necessary degree as compared with the smaller amount of energy which has to be put into the

* *Journal of the Iron and Steel Institute*, 1947, vol. 156, May, pp. 98–118.

water in a steam boiler. As against that, we must of necessity reject the latent heat in the steam in a steam turbine, whereas we are not compelled to reject latent heat in the gas turbine.

As has been said over and over again, and I repeat it now, the possibility of using a gas turbine depends on the success which has attended rotary-compressor design, and the developments in heat- and creep-resisting materials. These statements are axiomatic, but we cannot think of them often enough. Had these factors been resolved years ago, I am sure that the many hundreds of patents taken out since 1790 on gas turbines would have been effective long before this.

We know that the compressor design as such has now been more or less resolved. It is reasonably straightforward to make an axial-flow compressor with an adiabatic efficiency of 83–84%. It is rather difficult to squeeze the additional 2–3% out of it, but it is a reasonable basis on which to start a gas turbine, with a compressor efficiency of 83%.

That leaves us with the one indefinable—the behaviour of heat-resisting materials. As I see it, I would classify the use of these into three groups. We have a great deal of experience in the past which helps us to believe that we can with confidence use ferritic steels up to temperatures of 550° C. Again, there is good reason to assume that we can use, with equal reliability, for temperatures up to 600° C. austenitic steels of the 18/8 modified or stabilized types. The real crux of the matter, however, is the use of steels of the heat- and creep-resisting types for temperatures of the order of 650° C. Here we must have recourse to some of the special steels of which we have very little knowledge at present. As far as I know, of the steels which we should like to use for temperatures of this order for rotor and cylinder construction, I doubt whether more than 3000–4000 hr. of creep tests have been done on them ; and designing, as we do industrially, for lives of the order of 100,000 hr., we have nothing to go on but extrapolation, and extrapolation from 3000 to 100,000 hr. is very dangerous, to say the least of it. At the present time we must walk warily with regard to the use of heat- and creep-resisting steels at temperatures of the order of 650° C., design conservatively, and work on reasonably low stresses, if we are to expect the life which we usually get from a steam-turbine unit.

As to the cycle on which our gas turbines are going to work, we in this country, and for that matter people in most other countries, have had experience so far only on open-cycle units, that is, units in which the fuel is burned directly in the air stream. I do not propose to speak of the closed-cycle units, of which we have had no experience ourselves, and on which there is still only very limited experience anywhere in the world. That they have a part to play goes without saying, but they have the drawback that the inherent simplicity of the open cycle is lost when one has to consider what is really an air boiler instead of a steam boiler. The idea of an air boiler is not one which appeals to most engineers very much, to put it mildly. In any case, I am not proposing to speak of the closed cycle, and our remarks are confined to the open-cycle turbine, on which we have had some experience and on which there is considerable experience elsewhere to draw upon.

As far as fuels are concerned, we limit our consideration to the use of blast-furnace or coke-oven gas, to a mixture of these two gases, or to oil fuel. When we talk of oil fuel, I prefer at the moment to talk of a distillate and not a residual. That we shall be able eventually to burn residuals goes without saying, but a good deal of work has to be done before we can do so with confidence. We can, however, with confidence say that we can burn distillates, which carry the burden of about 25% greater cost than the residuals.

I deprecate the loose remarks made in this country and in the world in general on fuels. They are variously described as " heavy," " crude," " Bunker C," and by other names which have no meaning. " Bunker C " will embrace kerosene within its specification, so that it is not enough to talk about Bunker C. I would, therefore, put members on their guard against loose talk on fuels which can be burned in gas turbines, and for our part we propose to talk of distillates.

I think that at this stage we might look at Fig. 1 and see what a simple gas turbine looks like. This shows the simplest type of gas turbine with a reasonable working efficiency. It is the simplest type that one could conceive, except perhaps a turbine suitable only for standby, where efficiency is not such an important point. In this turbine we have the main components, consisting of the compressor, the turbine, the heat exchanger, and the combustion chamber. These are the four essentials of the simplest type of gas turbine which can profess a reasonable efficiency.

If we trace the flow of the working medium, we come first to a filter house. It is essential to filter the air, because oily dust very quickly reduces the efficiency of the axial-flow compressor. We have used a " Viscous " type of filter quite successfully, and I see no reason why this should not be used in practice. It is simple and effective, and after it gets a light coating of dust it is very effective indeed.

The axial compressor is almost axiomatically the type which is used on a gas turbine, though for certain installations, particularly of small

power, where we are not dealing with such large volumes, it may be just as economical from an efficiency point of view, and probably cheaper in first cost, to instal a centrifugal compressor which in certain circumstances allows the working line of the turbine to coincide much more closely with the maximum working efficiency of the compressor than does the use of an axial-flow compressor ; but these occasions are few.

As I have already said, the efficient design of axial-flow compressors has now been reasonably well established, and at the present time we are mainly concerned with reducing the costs by seeking for improved methods of making the rather complicated and expensive blades which are essential to its efficient working. We may use stainless iron, aluminium bronze, or Monel for the blades. Where there is danger of corrosion we do not usually employ aluminium or aluminium alloys. It is a straightforward matter, by stamping to finished dimensions, or by stamping to rough dimensions with final machining, or by machining from the bar, to make the type of blades—twisted, flattened, and tapered — which are essential to the efficiency of this compressor, and with improved machining and stamping methods these blades will eventually come down to a reasonable commercial cost.

We then come to the heat exchanger. The heat exchanger is the life-blood of an efficient gas turbine. If it were endowed with human qualities and feelings, the gas turbine would, I imagine, say to the heat exchanger, in sympathy with the love-sick swain in one of Margaret Kennedy's *Songs of the Hebrides* :

> " As the leaf is to the tree,
> As the Summer to the bee,
> So art thou, my love, to me."

From the heat exchanger the hot air now passes into the combustion chamber, where it mixes with the gas which has been compressed in a separate gas compressor, and combustion takes place. We consider that it is possible, with proper design, to burn this gas efficiently in what is now known as the double-shell type of combustion chamber without the intermediary of firebrick to save the walls. If this can be done—and we feel fairly sure that it can be—it prevents the danger of scaling of the firebrick and the consequent erosion of the turbine blades ; because we must always remember that in the open cycle the working medium must be acceptable to the turbine in so far as dirt, erosive particles, and so on, are concerned.

The gas is intimately mixed—that is essential—with the air, and I should have said that in a practical job the gas itself will be heated in part of the heat exchanger, or separately, so that the gas and air are entering hot ; and, as was mentioned this morning, that is probably very desirable.

The gases then pass into the turbine, and exhaust as before. Considering the turbine as such, conditions are favourable to good efficiencies, because, on account of the large mass flow, good blade heights and therefore good efficiencies are obtained. The turbine design calls for modification from existing steam-turbine design, on account of the high temperatures which are used, and it is more or less essential, as far as the cylinder is concerned, to dissociate the outlet and inlet belts from the main body of the cylinder, so that they do not control the expansion or distortion of the cylinder body. So far as the spindle is concerned, it is probable that it will usually be of disc construction, though for moderate temperatures, of the order of 1125–1150° F., it is possible to make quite large rotors in some of the modified 18/8 steels which will have a life comparable to that in a steam turbine. For higher temperatures, however, it is difficult to obtain sound forgings in the special heat- and creep-resisting materials, and until those developments have been more advanced we shall probably be well advised to go in for disc construction.

As I have already said, this represents the simplest unit which one can conceive, and is capable of outputs up to 5000 kW. It is independent of cooling water, except that used for oil cooling and the like. But for larger powers, and even for powers of about 5000 kW., it will generally pay to split up the compression, shown in Fig. 1 in one stage, into two or more stages, so that when water is available I suggest that we should use it to increase the efficiency and to reduce the size of the plant, both of which the use of cooling water makes it possible to do.

Figure 2 shows a layout of a simple plant as described above. The air from the filter house passes through the compressor and then goes through the heat exchanger, but in this installation the heat exchanger is split into four parts, with a double exhaust from the turbine and a double inlet into the compressor. It then passes into the combustion chamber, out of the chamber, and so to the chimney.

I do not think that the layout calls for much further explanation, except perhaps to say something about the heat exchanger itself. There has probably been more hot air talked about heat-exchanger design than about the design of any other component, and designs have been put forward for large tubes, for small tubes down to $\frac{1}{8}$-in. dia., regenerative types, and so on. The view which we take for a heat exchanger for normal work is that the body and tubes can be made of mild steel. The heat exchanger shown need not be in the basement, and could just as

conveniently be outside the building ; and therefore, being made of mild steel and capable of being put outside the building, bulk and weight do not matter so much in its construction as may be imagined ; and the only benefit of using small tubes is to cut down the size, and particularly the length. On the other hand, we never get " owt for nowt," and if we use small tubes, cleaning becomes more difficult. With the large tubes, $\frac{5}{8}$ or $\frac{3}{4}$ in. in dia., built largely on condenser practice with a floating head, the tubes are easily cleaned, as has been found in our own experience.

The plant has generally to be shut down to clean the tubes, unless a duplicate heat exchanger is fitted, but as a rule there is no difficulty in shutting down the plant for cleaning if it is known when it is going to be shut down. With our own plant we have run for six months before cleaning the heat exchanger, and even then it was not really necessary—we wanted to have a look at it, and we cleaned it at the same time. Using large tubes, we do have the benefit, we claim, of relatively easy cleaning. In this case, with the large tubes the gas goes through the tubes and the air circulates outside ; with the smaller tube designs the air goes through the tubes and the gas outside. It is therefore necessary to get rid of the deposits which become deposited on very closely spaced small tubes, and I do not know of any type of soot blower which will get rid of the deposits on such a compact bundle.

It is essential in the interests of economy to cut down the size of plant whenever possible, and to use when possible interstage compression with intercooling. Figure 3 shows the effect which one stage of intercooling has both on the specific output of the plant expressed in B.Th.U. per pound of air in circulation and upon the efficiency. The efficiency with one stage of intercooling has risen, as is shown, from a maximum of about 25 to something of the order of 27·75%. That is quite an appreciable gain, but it falls into insignificance compared with the increase in specific output, which has been increased from about 35 up to about 46 B.Th.U./lb.—an increase of about 33%. The effect on the specific output is therefore to increase it by some 33% for one stage of intercooling, and therefore wherever possible we adopt this principle of splitting the compression and using intercoolers.

Figure 4 shows a plant, very similar to the first one, for an output of approximately 5000 kW., but with the introduction of an intercooler, the compression having been split into two stages. The air, as before, comes from the filter house, and passes through the L.P. compressor and then through the cooler, which can conveniently be split into two banks, one lying on each side of the turbine, so that the tubes can be withdrawn vertically for ease of handling. After passing through the second or H.P. compressor stage the air passes through the heat exchanger as before and into the combustion chamber.

When considering bigger outputs, in excess of 5000 kW., say, outputs of the order of 10,000 kW., it is necessary to depart from the simplicity of the units which have been discussed up to now. In a constant-speed unit substantially the same volume of air is in circulation independently of load, and load control can be effected mainly by temperature variation. Bigger units, therefore, are usually split into two or three lines, whereby the compressor load is dissociated entirely or partly from the alternator load. This allows of an independent speed of the compressor line, and the delivery of a greater or less amount of air to the turbine according to the load which it is carrying.

Figure 5 shows the layout of such a unit. We have substantially split the compressor line from the power line, although in this case the H.P. compressor is attached to the power line to provide a better distribution of power between the two lines. Although it detracts from the overall efficiency to some degree, in that the compressor line is not completely independent of the power line, it does not have a big effect, and generally speaking the layout is quite economic. Here, as before, we intercool between the L.P. and the I.P., and again between the I.P. and the H.P. We deliver from the H.P. to the combustion chamber as before, and hence through the high-pressure turbine ; but we introduce a new feature, a second combustion chamber, known as a reheat chamber, whereby the gases, which still contain a certain amount of oxygen, receive a further quota of fuel, and are in general reheated at the entry to the L.P. turbine to substantially the same temperature at which they entered the H.P. turbine. They then pass, as before, through the heat exchanger. Again the gas is compressed by a compressor driven off the compressor line. Bleeding takes place at the point shown, at a pressure suited to the reheat chamber.

Considering this gas compressor in a little more detail, although the amount of gas with which it is dealing is small in comparison with the total amount of air, it is nevertheless not negligible, and in the designs which are put forward for consideration we have incorporated a centrifugal compressor, knowing beforehand that we are sacrificing efficiency to what we hope is increased reliability. It may happen that with the best cleaning in the world deposits will come over in the gas stream, and the centrifugal compressor is less sensitive than the axial. We purposely, therefore, sacrifice efficiency for what we hope is

increased reliability, and that, I think, is justified, particularly in a steelworks, where one or two points of reliability are worth quite a number of points of efficiency.

Figure 6 shows the benefit of the reheat in efficiency and in output. It was previously shown that one stage of intercooling was capable of raising the efficiency by some two points, and of increasing the specific output by some 33%. Here we have the effect of reheat, which we incorporate in a two-line job for larger powers. The thermal efficiency has risen from 25% to 27%, and the specific output has risen from some 34–35 up to 48 B.Th.U./lb.—again something of the order of a 33% increase and hence a decrease in the size of the unit. Both intercooling and reheat, therefore, appear to pay handsome dividends, and for a large job it is not considered that a gas turbine has much chance of competing with a steam turbine if it does not incorporate these two principles.

Control has been dealt with very briefly in the paper. It is not difficult, but it is perhaps not so easy as one would imagine from reading the paper, since in order to get delicacy of response, it may be necessary to consider an unloading valve in the air line for a very sudden reduction in load. Generally speaking, the load does not rise quickly, but it often falls off quickly, and it is the falling-load characteristic which is our greatest concern in a gas turbine. It may be that owing to the sluggishness of the response, because of the heat capacity of the components, the delicacy is not sufficient by merely turning off the fuel, or, in the case of a two-line job, by cutting down the speed of the compressor turbine, and therefore the flow to the working turbine, as well as cutting down the temperature, and in such a case a sudden reduction in load would be taken care of by the unloading valve.

I have given only a brief description of gas-turbine power plants, and at this stage I must ask my colleagues to deal with specific applications of the gas turbine in a steelworks, a matter in which we are all profoundly interested at the moment, having regard to the interest which has now arisen in axial compressors for blowing, as distinct from the centrifugal type.

Mr. **W. H. Gibson** (Messrs. C. A. Parsons and Co., Ltd.), who presented Part II of the paper (" Blast-Furnace Blowing Applications "), Section I (" The Axial Blower "), said : In this paper I propose to deal with the application of the axial-flow compressor to blast-furnace blowing. For convenience, in this and the following sections of the paper we shall consider a blast-furnace with the following blowing demands. First, we have chosen a normal blowing demand of 55,000 cu. ft./min. at 16½ lb./sq. in. (gauge). The next condition is that of maximum pressure, which we have assumed to be 48,000 cu. ft./min. at 21 lb./sq. in. (gauge). The third condition is that of maximum volume, such as may be required later in the furnace life, and for this we have allowed a 25% increase on the normal volume. This gives us a maximum volume of 66,000 cu. ft./min. The other conditions which we must bear in mind with regard to blast-furnace blowing are the low pressures and volumes required when blowing-in a new furnace or under conditions of reduced trade.

These conditions have been adequately met by the combination of a centrifugal compressor and steam turbine. However, the centrifugal compressor has a maximum efficiency of about 75%, and, as we all know, owing to the recent research into axial-compressor design the axial-flow compressor now has a maximum efficiency of the order of 83–84%. Apart, therefore, from providing an introduction to subsequent sections dealing with a particular type of axial compressor for blast-furnace blowing, we shall consider the straight axial compressor as a blast-furnace blower.

Figure 7 of the paper shows an axial-flow compressor. This compressor is designed for only 10,000 cu. ft./min., and runs at 6000 r.p.m. The delivery pressure is 41 lb./sq. in. (gauge), which is much greater than would be required for a blast-furnace. The air enters through the volute in front and passes through successive stages of stationary and moving blading. We define one " stage " of blading as one moving row and one stationary row, and this compressor has 25 stages of blading.

To achieve our conditions of 55,000 cu. ft./min. for the furnace which we have in mind, we should need a much larger compressor, but at the same time, since we require only 16½ lb./sq. in. delivery pressure, we should require only half the number of stages, *i.e.*, between 10 and 15, and the speed, of course, would be correspondingly lower, probably about 3000 r.p.m., so that our blast-furnace axial-flow blower would be in geometrical proportions much fatter and considerably shorter than this one. As a matter of interest, this axial-flow compressor is used in Parsons' experimental gas-turbine unit. It is directly coupled to the gas turbine in the background (Fig. 7).

For the attainment of high efficiencies in axial compressors, the blading requires to be twisted. In this compressor we use a design of blading which we call " straight stator "; that is, only the moving or rotor blades are twisted. The blading is manufactured from rolled strip, cut to length, and fixed by methods employed in steam-turbine practice.

Figure 8 shows the pressure/volume characteristic of an axial-flow compressor suitable for blast-furnace blowing. The maximum efficiency given by a centrifugal compressor is, as I have said, of the order of 75%. The lines shown are called " speed lines," that is, lines of constant speed, while the others are lines of constant adiabatic efficiency. For each " speed line," as the compressor is throttled the volume falls off and the pressure rises, until eventually a point is reached which is called the " surge point," this being the natural pumping limit of the compressor. This compressor attains its maximum efficiencies at the surge point, and it is of the order of 84½% at normal speed, falling off slightly at higher and at lower speeds.

However, if this axial-flow compressor is compared with the centrifugal compressor it will be seen that there is a considerable range of both pressure and volume where it is considerably more efficient than the centrifugal compressor. We have chosen our conditions of 55,000 cu. ft./min. as 100% blast-air volume, and also the gauge pressure of 16½ lb./sq. in. as 100% blast-air pressure. It will be seen that, working more efficiently than the centrifugal compressor, it is possible to have a variation of blast pressure from 78% right up to about 137%, and also working at a constant pressure and at 100% speed the volume can be varied from 110% down to a lower volume, the detailed consideration of which is very interesting. The maximum efficiency of this axial-flow compressor is 84½%, and we can afford to blow off a considerable volume before the equivalent efficiency of this compressor falls to 75%, which has been taken as representative of the centrifugal compressor. It is seen, therefore, that the effective working range of a low-stagger axial compressor is much greater than is generally appreciated, and a considerable proportion of this working range is at a much higher efficiency than that of the typical centrifugal compressor.

I should like to say a word about the method of design of axial compressors. This has been fully dealt with in an excellent paper by H. Constant and A. R. Howell ; their particular application was aircraft axial compressors, but almost the same method is used for these heavy industrial axial-flow compressors. As I have mentioned, in order to attain the high efficiencies associated with the axial-flow compressor it is necessary to twist the rotor blading, and in some designs it may be necessary to twist both rotor and stator blading. The chief problem at the present time is, therefore, to produce a blading which will give a high efficiency of about 84% and at the same time keep the cost to a reasonable figure.

Figure 9 shows the horse-power/volume characteristic of the same compressor. As the compressor is throttled and the flow reduced and the speed is constant, the horse-power falls off only slightly. At the beginning of my remarks, I mentioned that we may have to make provision for at least a 25% increase in blast-air volume, which may be brought about by modifications to the furnace, and in order to obtain this 25% increase over the normal blowing demand an increase would be required of about 40% in the horse-power available from the driving machine.

Figure 10 brings out fairly clearly the difference in bulk and proportions between a typical multi-stage centrifugal compressor and an axial compressor. Both are designed for the same duty at the same speed, in this case 5000 r.p.m. I must apologize for the fact that these compressors were designed for a much higher compression ratio than would be used for blast-furnace blowing, but that fact in no way invalidates the argument. It will be seen that about three stages of axial-flow blading are equivalent to each impeller of the multi-stage centrifugal compressor. It is hoped that when the cost of the blading is reduced, the overall cost of an axial compressor will be slightly less than the cost of a centrifugal compressor designed for the same duty.

We have discussed the application of the axial-flow compressor to blast-furnace blowing, but the problem of driving the axial-flow compressor for blast-furnace-blowing purposes also arises. We have seen that the simple gas-turbine cycle can attain efficiencies of the order of 22%, using blast-furnace gas as fuel, and I think that this figure is comparable with the best efficiencies obtainable from a steam plant such as is used in the iron and steel industry. The gas turbine must therefore be considered as an alternative to the steam turbine as a drive for the axial-flow compressor or the centrifugal compressor used for blowing a blast-furnace.

The simplest type of cycle, in which the axial compressor for the blast-furnace is coupled directly to the same shaft as the main compressor and turbine unit, is very limited in its speed range ; in fact, using an axial compressor it is not possible to run the whole unit at very much below 70% of its design speed. We are therefore forced to separate the turbine which is supplying power to the main compressors and the power turbine which is driving the blast-furnace compressor, in order to obtain the flexibility of speed required by the blast-furnace compressor. Such a plant is shown in Fig. 11. The air enters the first stage of compression, passes through the intercooler, the second stage of compression, and the heat exchanger into the combustion chamber. At this point blast-furnace gas is introduced, after passing through its own individual compressors and intercoolers, and the burnt mixture passes through the

compressor turbine, the power turbine, and the heat exchanger to the exhaust.

Unfortunately, in order to obtain flexibility of operation we have had to increase the number of main components with consequent increase in cost.

The question therefore arises, " Is it possible to combine the gas turbine with its many advantages and the axial blowing compressor with its inherently high efficiency, without introducing the complication and increased cost of a two-line unit ? "

We believe that a single-shaft gas turbine coupled to a bled-axial compressor which meets both blast-furnace and combustion-air requirements offers an attractive solution. This will be dealt with by Mr. Railly in the following section.

Mr. **J. W. Railly** (Messrs. C. A. Parsons and Co., Ltd.), who presented Section II (" The Bled-Axial Gas-Turbine Blower") of Part II of the paper, said: My colleague Mr. Gibson has already pointed out the disadvantages of the axial compressor driven separately, either by a steam turbine or by a gas-turbine unit. He has shown that this involves a two-line unit, with the gas turbine driving its own compressor and a separate turbine driving the compressor for the blast-furnace air. The bled-axial arrangement overcomes these difficulties. The main advantage of the bled-axial blower is the economy in the number of machines, and therefore the economy in space.

From Fig. 12 it will be seen that the compressor for the gas turbine is incorporated with the compressor for the blast-furnace air, so that the compressor for the blast-furnace air is combined with the compressor for the gas-turbine charge. The air enters where shown, passes through the compressor and enters the heat exchanger, where it gets the heat from the exhaust gases of the turbine, leaves the heat exchanger and enters the combustion chamber, where the blast-furnace gas, compressed in a separate gas compressor, is injected. The hot gases pass through the turbine and out through the heat exchanger. It is the most simple type of gas-turbine cycle, and the air for the blast-furnace is compressed within the compressor for the gas turbine.

The advantages of this unit I have outlined already, but the most useful, apart from economy, is the wide range of operation possible with this type of machine. This advantage follows from the fact that the turbine must necessarily have a very large volume of air in relation to the volume of air which passes to the blast-furnace, so that the proportion of the compressor used for both is not very much larger than it would be if it did not compress the blast-furnace air.

It is necessary to assume some figures of conditions to give an idea of how the bled-axial compressor meets those conditions, and so a normal condition has been chosen of 55,000 cu. ft./min. of free air delivered at a delivery pressure of 16½ lb./sq. in. (gauge), the same machine to supply 66,000 cu. ft./min. as the maximum condition at about the same pressure, and about 48,000 cu. ft./min. at a pressure somewhat in excess of 16½ lb./sq. in., say, of the order of 21 lb./sq. in. (gauge). For such a condition of 55,000 cu. ft./min. we would employ a turbine with a top temperature of about 1100° F. and a heat exchanger with a thermal ratio of about 80%.

I wish to draw attention to the necessity for the portion (2) of the compressor in addition to the portion (1). The pressure ratio of the compressor has an optimum value for any given type of cycle, and in this type of cycle with the top temperature envisaged the pressure ratio is of the order of 3. Below that pressure ratio, the efficiency falls, and similarly above it the efficiency again falls, so that we must not be very far from a pressure ratio of 3 at the normal condition. For this reason, the gas must be bled off at an intermediate part of the casing in order to meet the condition required by the blast-furnace of 16½ lb./sq. in. pressure. This is done by providing an annular break in the casing whereby the air is bled off and the remaining air passes through a few more stages.

I want to mention the effect of specific output. The specific output of a gas-turbine cycle is very much lower than that of an oil engine of the same power. The gas/air ratios are much smaller than are required by a gas engine ; hence to meet a given power there must be quite a large volume of air. We have already dealt with methods of raising the specific output, *i.e.*, decreasing the size of machines. This can be done by raising the top temperature above 1100° F., but there are limitations in that direction, as Dr. Bowden has already explained. Another way of increasing the specific output, and therefore of reducing the size, is to intercool. Intercooling increases the efficiency and also increases the specific output ; in fact, the total mass flow is decreased in the ratio of about 1/1·33, or in other words about 70%, of its previous value for the same top temperature.

Another way of decreasing the mass flow for a given power output is to break up the turbine expansion into two stages and put in a combustion chamber. However, there is a further effect of specific output. It can be shown that the ratio of the bled air to the air going through the turbine is proportional to the specific output ; in other words, the larger the specific output the larger is that ratio. This has a sharp effect on the characteristics of the machine, tending to restrict

the range of operation the higher the bled-off ratio becomes.

We have calculated the operating characteristics for this type of machine, and Fig. 14 shows the characteristics of the first stage of the bled-axial compressor. Here again lines of constant speed are shown, and in this case the normal speed is 87·5% as a percentage of the top speed of 100. Along the abscissa is the total inhaled volume for the first stage, and the ordinate is the adiabatic temperature rise or its equivalent in delivery pressure in pounds per square inch. This, of course, relates to the total air inhaled by the first stage, of which only 55,000 cu. ft./min. in the normal condition are passed to the blast-furnace, so that 142,000 cu. ft./min. are handled by the first stage, of which 55,000 cu. ft./min. go to the blast-furnace. The advantages of this are at once obvious, because a small change in the total mass flow inhaled by this machine results in a very large change in the amount of air going into the blast-furnace. That is the real advantage of the bled-axial compressor, to have this wide range in the amount going to the blast-furnace from small variations in the compressor itself.

Another advantage lies in the wide range of possible delivery pressures. The lines of constant volume run approximately parallel to the surge line and to the upper limit of temperature, so that simply by varying the speed it is possible to obtain a wide range of pressure at a constant volume. A line is indicated beyond which we cannot move, because of the restrictions of inlet temperature to the turbine.

These characteristics have been plotted in a way which will be more familiar, namely, delivery pressure as ordinate and inhaled volume to the blast-furnace in cubic feet of free air per minute as the abscissa. Here again we have lines of constant speed, and the " wiggly " line shows what happens to the surge line in these conditions (Fig. 15). The similarity between the bled-axial-compressor characteristics and the centrifugal-compressor characteristics is now obvious, because these lines of constant speed have become almost constant-pressure lines, and it is possible to have a maximum volume of 66,000 cu. ft./min. and a minimum volume of 25,000 cu. ft./min. for this machine, which is about as good as can be obtained with the centrifugal compressors which are produced today without discharge vanes.

The question of the control of this compressor will be dealt with by Mr. Voysey, but one question is whether we shall use automatic means to prevent the compressor from moving to the right of this line* or whether we shall do it by hand, when a warning device operates to show the position.

* Indicated on screen.

I would draw attention to the trend of thermal efficiency. With the normal position of 55,000 cu. ft./min. delivered to the furnace at a pressure of 16½ lb./sq. in., the thermal efficiency is of the order of 21·2%. Unfortunately, because of the margin which we must have, this is lower than could be obtained. When the furnace is approaching the end of its life we may have an efficiency of 23½%, but at present, to make provision for this 66,000 cu. ft./min. which may be required, we have to operate at a lower temperature, and therefore have a lower efficiency, of the order of 21%.

In conclusion, I should like to recapitulate the advantages of this machine. They are : Economy in machinery, and particularly in space, there being only three machines required—one compressor, one turbine, and a gas compressor, together with a small starting motor and a heat exchanger. The overall efficiency compares favourably with other schemes of providing blast-furnace air. Finally, it is only slightly inferior in regard to operating characteristics to existing centrifugal machines.

Mr. **R. G. Voysey** (Messrs. C. A. Parsons and Co., Ltd.), who presented Section III (" Operation and Control of the Bled-Axial Gas-Turbine Blower ") of Part II of the paper, said : The previous speakers have brought out the point that one of the most interesting and possibly immediate applications of the gas turbine is for blast-furnace blowing, with a simple layout wherein the normal compressor of the turbine is bled to provide the air for the blast-furnace. Figure 16 shows the possible layout of such a plant, and is not unlike some of the foregoing layouts. The machine is arranged more or less in one line, with the combustion chambers and heat exchangers split into two lines lying symmetrically alongside. Unlike the previous layout, there is a bleed from the middle of the compressor ; otherwise the flow path is much the same.

Figure 17 shows the characteristics mentioned by the previous speaker, with some lines added. These are the characteristics of the blower as seen by the furnace. The volumes are the quantities demanded by the furnace, not those going into the inlet of the compressor, which, of course, are of more importance to the designer. This is the best jumping-off point for considering any special difficulties of control which arise. Naturally the point of view of the user is what matters in discussing controls, and the intention in discussing the matter is to see whether any special difficulties arise which are not normally encountered with a steam-turbine plant.

For this purpose we should consider the various duties on which the plant will operate. Its first and obvious duty is to operate at 55,000 cu. ft./min. and 16½ lb./sq. in., as assumed by previous

speakers. It is imagined that under conditions of a hanging charge the pressure may have to rise to 21 lb./sq. in. and the delivery fall off to 48,000 cu. ft./min., and that the case must also be met of an extra delivery up to 66,000 cu. ft./min., to allow for wear and tear during the life of the furnace.

Two conditions cannot be shown there. There is the condition when it is desired to remove temporarily the full pressure and volume of flow to the furnace to allow the dropping of a hanging charge, and there is the even more extreme case of the blowing-in of the furnace, where quantities as low as 10,000 or 15,000 cu. ft./min. and pressures of about 5 lb./sq. in. may be desired. These fall outside the normal characteristic of the blower and can be met only by bleeding ; but there is no insuperable difficulty in this. If you look at the limits of the area within which we can work, you will see that there is a top boundary of speed, there is the surge boundary, and on the right and left there are bounds of temperature ; one is 1200° F., or about 650° C. In a steam-turbine-driven blower it is possible to lower the pressure and volume almost indefinitely by dropping the speed, but if we do it in this case we shall run into these lines of high temperature ; we shall be running the plant at so low a speed that, with its given component efficiencies, it will have great difficulty even in driving itself, and we have to resort to high temperatures. This lower region, therefore, is a forbidden region, and the control must make some effort to get round this. It can be said, therefore, that the two most difficult cases are the normal snorting of the furnace and the running in. It is fortunate that these conditions occur for only a limited time in the life of the plant, and therefore efficiency under these conditions is not too important.

Figure 18 is a schematic layout which shows what may be expected from the simplest of control schemes. These suggested controls have been arrived at after some discussions with the staffs of steel companies, and represent a skeleton for discussion. We have tried to visualize every case put forward, but others can always be considered. The main components of the gas turbine are shown by heavy lines, and we will say no more of those, but go on to consider the various controls, which in this case are imagined to operate by throttling the gas line. They could equally well be imagined to operate by by-passing the gas line from the gas compressor, or to work with a liquid-fuel system.

Let us take the ones to which you are most accustomed. The first one of importance, No. 7, is the usual furnace snort valve, whereby the furnaceman, by a manually operated valve, can so lower the pressure and volume as to secure the drop of the hanging charge. There is nothing very noteworthy about it. In a steam plant it could be one of two things. If the plant is on a constant-speed basis, we must arrange to throttle the pressure and by-pass the air, so as to secure that the plant is delivering the required low pressure and volume without seriously disturbing the normal delivery conditions at the turbine. If there is a constant-volume governing system, then normally there is only a bleed provided, and the overloading effect on the turbine automatically lowers the speed of the turbine to give the desired pressures. I have shown there a double valve to avoid the pitfall of running into the lower region of bottom temperature. If we bled only, without trying to restrict the pressure, the turbine would make every effort possible to give the pressure and volume, and would speedily descend into that lower-temperature region.

There is the anti-surge control, to prevent wandering into the forbidden region. There is also a top-speed control, No. 8, which could if desired be set by the customer. Used in that way, the plant could be run as a constant-pressure machine, because constant speed is roughly constant-pressure working. There is the usual overspeed trip, which is an emergency valve only, invoked in special situations. There is a top-temperature control, designed to ensure that the top limit of temperature which the designer has decided is safe for the plant shall not be exceeded for more than a few seconds. Finally, a valve which deserves consideration is what would normally be the constant-volume control of the plant. It measures a volume taken from the bleed, and it also measures the pressure of that bleed, and combines them in the manner desired by the furnaceman.

Let us look back at the characteristics, so that we can discuss this valve a little further. In Fig. 17 delivery pressure is plotted against volume, and we can imagine that we are operating at the normal point. If for any reason the furnace showed any tendency to choke, then the instinctive tendency of the machine, quite apart from any governing action, is, of course, to carry on putting in the same fuel, so that we may expect to travel along a line of constant fuel. In this case it happens to be 10 lb./sec., so that the tendency of the machine is to travel in the direction of the arrow A. This is more or less a constant-pressure characteristic, and therefore, if we imagine the machine working on that principle of constant fuel setting, it would be under constant-pressure blowing conditions. I understand that it is usually more desirable to have the converse of a constant-volume flow. In that case the constant-volume control would be set to manipulate the fuel so as to ensure a rise in the

direction 1–B. If it were desired, we could in the control gear provide any method of getting any desired locus between this normal point 1 and the condition of the worst case of hanging charge, 2.

On the previous figure it was referred to not as constant-volume control but as constant-mass control. In normal operations it is required to operate the blast-furnaces under steady conditions, and steady conditions correspond to steady mass reactions. It is imaginable that you would like as an ideal, constant mass flow. Working under normal conditions, constant mass flow is very nearly constant volume, so that a control which gives constant volume is good enough.

Unfortunately, in our case we have a double flow going into the compressor ; there is the flow required for the blast-furnace and the flow required for the turbine, and so a measurement of volume taken there does not give a fair impression of what the blast-furnace is getting. It is necessary, therefore, to put a Venturi meter which measures the volume flow on the outlet of the compressor, just where the blast is taken off. By the very nature of the problem this is the point at which the pressure and temperature should oscillate much more widely on a percentage basis than one expects to find at the inlet to the compressor, and so it may be necessary in such a case to put in a refinement which is never necessary in ordinary plant, and that is to make the constant regulator responsive not to. constant volume but to constant mass. This is fairly easily done by taking a sample of the air through a small fan driven by a constant-speed motor. The pressure given by that fan responds, of course, to the density of the air, and can be compounded with the normal constant-volume control to give the constant-mass control which is probably the more desirable form.

We have not said much about cases 4 and 5, where we must snort or run at low volume and pressure for a considerable period for the running-in of the furnace. Let us take the last one, which is the hardest to meet, the blowing-in of the furnace. If we want only 5 lb./sq. in. to run in, we have on the graph the line for 5 lb./sq. in., and it will be seen that there is no margin at all as regards temperature, especially bearing in mind that this surge line is responsive to day temperature and day barometer and will show some wandering. We can only resort to a little throttling and deliver with our compressor 8 or 10 lb./sq. in. By that means there is a substantial margin between the surge line and the limiting temperatures between which we can operate without any danger of surge. Furthermore, we do not require this large volume, and therefore we must bleed. The case is very similar to the snort case ; we must throttle and bleed. It could conceivably be done by the normal furnaceman's manually operated valve, but it is better to provide a separate valve for the purpose, which will do it automatically, servo-operated, and relieve the furnaceman of any worries. The efficiencies would be slightly lower, owing to that throttling and by-passing, but inasmuch as the condition does not last for a long time in the whole life of the plant I do not think that is a very bad disadvantage.

In concluding this presentation, it is advisable to say that there are many applications of the gas turbine which have not been considered. There are plenty of them in the literature, and in the paper we have dealt with two. These two were concerned with attempts to get rid of regenerative stoves which are normally necessary to heat the gas. We have taken the stove away and replaced it with a heat exchanger. We have, as before, the compressor, the turbine, the normal combustion chamber, and the normal heat exchanger, but instead of bleeding-off air and passing it into stoves it has been passed into a heat exchanger which is fed by hot air coming from the combustion chamber. The combustion chamber, which formerly fed air direct to the turbine, now furnishes air to this auxiliary heat exchanger.

The whole point of the scheme is to try to save some of the losses which have occurred in a regenerative stove, and which can be, I understand, as high as 25% of the main throughput. In the case of a heat-exchanger design such as that shown, there is no loss of heat through radiation or convection from the outside, but only losses through the tubes from one line to the other. There is a control difficulty in operating such a scheme ; instead of having one set of controls to operate the turbine, and the furnaceman's controls to operate the blast-furnace, the two are badly entangled ; and the only thing to do in such a scheme is to leave the setting of this temperature to the furnaceman. A by-pass bleed must be added so that while the furnaceman controls the temperature it is still left open to the governing gear of the machine to mix in cold air so as to preserve the desired operating speeds and pressures of the blower.

Another possible scheme is instead of bleeding the compressor to bleed the turbine. This has a superficial simplicity very much like the idea of bleeding the compressor, but has the weakness that the controls become badly entangled, and it is not so easy to secure the furnaceman's temperature and his required blowing volume. There are various other schemes which could be put forward, but I wish as a concluding remark to say that it would be a pity if they prejudiced the other application which we have presented,

of a bled-axial compressor as a blower driven by a gas turbine and used as a blower for blast-furnaces.

Mr. **A. Bridge** (Appleby-Frodingham Steel Co., Ltd.) : I should like to express my appreciation to Dr. Bowden and his colleagues for their very interesting papers, and I am quite sure that all iron and steel works engineers will find these papers a very valuable work of reference.

It must be borne in mind, however, that when the gas turbine is adopted for blast-furnace blowing—and there is no doubt in my mind that it will be adopted, and probably much sooner than many of us realize—there must be an alternative fuel available for immediate use ; in other words, gas turbines should be designed for dual firing, because, if when using blast-furnace gas as a fuel there are operational difficulties at the furnaces, a major " shut down " may occur, affecting any, or in some instances all the blast-furnaces on the plant ; then the only way to " start up " again is by having an alternative fuel available to drive the gas turbine, unless of course there are standby steam-driven blowers available.

The bled-axial design of turbine seems to have great possibilities, and I am very much impressed by what I have heard today, as in this design of blower, the flexibility seems to be there, within the required range.

Conditions relative to " blowing-in " and " casting times " have been mentioned, and the bled-axial design would appear to be quite capable of meeting these conditions.

I was very pleased to hear Dr. Bowden say that reliability was more important than the last ounce of efficiency, because of all the ancillary equipment in connection with any blast-furnace, the first essential is an absolutely reliable blowing unit ; this is vital not only to production, but also for the safety of personnel and plant.

Mr. **R. J. Welsh** : (English Electric Co., Ltd.) : I have listened to these papers with much interest and find myself in fair agreement with the authors in regard to almost the whole of their technical statements and deductions. I do not, however, agree with the policy which their conclusions have led them to recommend. In brief, I do not believe that the time is yet ripe for the building of a bled-axial gas-turbine blower for blast-furnace work.

Of the several reasons which lead me to this view, I will mention only two. The first reason is that in the production of any new size or type of gas turbine, a very large proportion of the work may be expected to lie in the development of the compressor, and in my opinion any gas-turbine manufacturer ought to wait until he has successfully built a number of large gas-turbine compressors of normal type before trying his hand at the more complicated problem of the bled-axial machine.

To produce a successful gas turbine of even the simplest type at the present state of the art it is necessary for a designer to put all he knows into the compressor. Even after he has done so he must be prepared to face months of testing and modifications before the machine will give its desired performance. When it comes to producing a bled-axial compressor the designer is, as it were, working with one hand tied behind his back. Instead of working constantly for the best efficiency he must make a series of compromises between conflicting requirements and, unless he is extremely lucky, he will be faced with the choice between a long and costly testing and development programme or the acceptance of a mediocre performance. This being so, and no gas-turbine designer can honestly deny that it is so, then, remembering that the gas turbine has yet to prove its worth in any field of industrial application, one cannot but question the wisdom of risking the whole reputation of gas turbines on so complicated an application before they have been thoroughly proved and found acceptable for more straightforward work.

My second reason for differing from the authors in their eulogy of bled-axial compressors is that, as they themselves have pointed out on pages 112 and 113, the advantages of this type of machine become progressively less with each advance in gas-turbine efficiency. It may well be that, by the time it is politic to embark on the design of a bled-axial machine, the case for this type of machine will have disappeared.

In my opinion, the correct line of approach for the application of gas turbines to blast-furnace blowing, a duty for which they ought to be very suitable, is to use standard power-producing gas turbines coupled to centrifugal blowers of approximately the same type as are at present employed on blast-furnace work. The gas turbines ought, of course, to be of a type having a separate output turbine, so that the blower can be driven with stability and good efficiency over a wide range of speed.

The development of the bled-axial machine ought to be regarded as the second stage of development, to be taken up only after the first stage has been successfully negotiated.

Mr. **T. W. Thursfield** (Messrs. Gjers Mills & Co., Ltd.) : I should like to thank the authors for bringing before blast-furnace operators something both new and interesting. Like Mr. Bridge, I have been wondering how it would be possible to start up a unit like this on a new plant. Some auxiliary supply will have to be available, because it will be necessary to have a small amount of wind to get some blast-furnace gas for a start ; and some day

the plant will have to be shut down and a fresh start made. I should like to know what the authors' suggestions are in that respect.

I note that the figures which they have taken as a basis are 55,000 cu. ft./min. at 16½ lb./sq. in. Those are more or less present requirements, but, as Mr. Bridge says, there is a possibility of furnaces requiring up to 80,000 cu. ft./min. On the other hand, it must be recognized that a different set of conditions prevail with furnaces making some special irons, which even in the distant future may require only something like 30,000–35,000 cu. ft./min. at 16½ lb./sq. in. If we were to have a unit such as has been suggested for a furnace of that kind, we should find that we were right in the surge region ; but no doubt some modification of design could be made to cover that point. It would be interesting to have an indication of the possible efficiency of a machine of smaller size such as I have mentioned.

In the paper the question is raised of discontinuing the use of stoves. We as blast-furnace operators are not particularly fond of stoves, and if some better appliance can be put forward to provide an interchange of heat so as to give us the required blast temperature we shall all be in favour of it ; but, when all is said and done, we are there not to make pig iron or slag or gas, but to make money, so we wonder what the cost of such an exchanger and its efficiency would be.

Mr. **B. Wood** (Messrs. Merz & McLellan) : The authors have given us an excellent review of this subject and have provided for some of us who are not very closely in touch with the operation of blast-furnaces some useful data on the conditions with which blowers have to contend. I feel, too, that Dr. Bowden in particular has shown very balanced judgment ; he has not been overpersuaded of the virtues of high temperatures, and therefore has not embarrassed some of us who do not think that it pays to talk " too red hot." It seems to me that the one firm which has had the biggest experience, and the only commercial experience in the world, is the one which uses the lowest temperatures, and therefore Dr. Bowden is in good company in talking of temperatures in the region of 1100–1150° F. as an absolute maximum. The possibility of going to 1200° F. later will be obvious, but, as all blast-furnace operators would point out, they are not greatly interested in the last ounce of efficiency, and if a certain efficiency can be obtained by refined design rather than by alarming temperatures, I feel that they would choose the refined design.

I am interested in the rather conservative figures quoted for compressor efficiency, 83–84%, which were the figures we started off with in 1938. There has been some advance since ; a recently published* test figure for a plant in Peru was 89%. It may be that if the blades are designed for a higher stage efficiency a steeper characteristic will be obtained. Perhaps the authors will elaborate on that point and tell us whether they have deliberately chosen a low stage efficiency in order to get a flatter characteristic.

With regard to the actual plant for the blast-furnace, I submit that the choice between the centrifugal compressor and the axial compressor for the gas is not on reliability, as has been suggested, but on a combination of efficiency and convenience. The gas compressor must at all times have a reserve of pressure in order to be able to respond to the control. since this is, in effect, the throttle ; it is what decides whether the turbine is going to speed up or slow down. There must therefore be a working margin. If this working margin is not used at any moment, the compressor has to be throttled or by-passed. When I worked this out some years ago, I found that whether you adopt throttle or by-pass regulation on an axial compressor you finish up about on a par with the centrifugal compressor and I think that is the practical reason for going to the centrifugal compressor for the gas.

Another point on which I am not clear is why we cannot have simply an axial compressor driven by a steam turbine. It has already been mentioned that the characteristic which the furnace really requires is a constant-volume one, and this is readily obtained with an axial compressor. It is always very difficult to find out from the literature, and even from talking to blast-furnace operators, what really is required. If it is true that a constant-volume characteristic is desirable, then the axial compressor seems to have an advantage immediately over the conventional centrifugal compressor. To regulate to another volume you would change the speed, and with a steam turbine this can be done without any considerations of excessive temperature or other difficulties. That does not apply when the compressor is driven by a gas turbine, as the authors have shown.

I am very interested in the revolution which has evidently taken place in the views of the team. A year or more ago, I came to the conclusion that the bled-axial compressor was the only workable solution for the blast-furnace application of the gas turbine, and I considered that the driving of a centrifugal compressor by a gas turbine, which was then suggested, offered us novelty without any advantage. I do not know what has caused this revolution. Presumably the gas turbine which was taken away from Germany is now in England and some data have been obtained from it, so we may have further information in time.

* *Power*, 1947, April, p. 68.

With regard to the starting of a gas turbine, the authors have pointed out that no gas turbine is self-starting ; they all have to be wound up by an auxiliary motor, so that not only fuel but also auxiliary power to the extent of about 5% of full load is required to motor it round so that it can start to produce some torque to run itself.

There is one minor point of nomenclature which I should like to mention. Mr. Gibson talked of a " stage," and he defined a " stage " as a reaction pair. That is the way in which I use the word, and therefore I deprecate the use of " stage " to mean cylinder. If we are going to talk about multi-cylinder gas turbines or compressors we should not call them " multi-stage," because they are all multi-stage.

A matter of some importance for the future is the next step of cutting out the Cowper stoves. If the gas turbine with bled-axial compressor can have its combustion chamber combined with the blast heater, very considerable fuel savings become possible. The limitation in this direction is the maximum blast temperature called for. If this is no higher than, say, 1000° F. then the steel blast heater presents no serious problem, though a by-pass, as the authors mention, is obviously necessary to maintain a sufficient degree of freedom at the turbine. It would be useful to have the views of furnace owners on whether they would be content with a temperature in the region of 1000° F., rather than 1500° F., which is a little too hot for the steel blast heater.

Mr. **J. Calderwood** (Sulzer Bros. (London), Ltd.): The two or three points which I wish to raise relate mainly to Dr. Bowden's part of the paper. The first concerns Figs. 3 and 6. I should like to ask him whether in the efficiencies plotted here he has included the losses, that is, the pressure-drop loss through the heat exchanger and in the intake and uptake pipes. It should be made clear, I think, whether that has been done or not. A second point on those two figures is that one shows the improvement with intercooling and the other shows the improvement with reheat. Could not a combined figure have been given showing the improvement when both are used ? As a rule they are both used together, and a mere simple arithmetical addition of the two curves does not show the combined effect.

One point which Dr. Bowden does not bring out, but which I think is most important to consider for the future, is the variation in efficiency with load. He tends to dismiss cycles other than the plain open cycle, but to my mind the main reason why people are working on different complex cycles or high-pressure cycles is that these all offer a much better partial-load efficiency. I should like to have a few comments from Dr. Bowden on that point, as to whether he does not think that in the future, particularly with larger plants, we are going to find that the closed or composite cycle will completely supplant the open cycle to get good partial-load efficiencies, or has he some secret up his sleeve for improving the partial-load efficiency of the open cycle ?

I agree very strongly with what he said about loose talk on the subject of fuels. I too have heard all sorts of loose expressions in defining fuels, and also suggestions that gas turbines can burn anything. Personally, I am of opinion that there is some reason to believe that in a few years' time they may be able to burn bad fuel, but nobody in this country, or anywhere in the world, has any real experience yet of a long period of operation on a really bad fuel. That is another problem that might come up with the blast-furnace gas turbine ; there will have to be effective cleaning of the gas before it is fed to the turbine, and a measure of the degree of cleaning required can only be reached as the outcome of a long period of service.

Mr. **F. E. Baumann** (Metropolitan-Vickers Electrical Co., Ltd.) : I should like to congratulate the authors on their most interesting paper ; I find myself in agreement with most technical points raised, but think that a number of their conclusions require further consideration. Their paper, as with other papers which have been published on gas turbines, serves to illustrate the great flexibility of the gas turbine. One can choose from a very wide variety of cycles and cycle conditions (according to the thermal efficiency required), but each particular choice will represent a certain capital cost and probable reliability, and every particular project should therefore be considered individually.

The analysis of the bled-axial compressor is extremely interesting, but it does appear to me that in practice the solution of control problems would involve considerable development. If in any application, as happens in the case of blast-furnaces, reliability is of great importance, one thing which has to be considered is the complexity or the simplicity, as the case may be, of the control system ; and that, to my mind, is one reason why a simpler plant should be considered as an alternative. I would look to the first blast-furnace installation as being a gas turbine driving a centrifugal compressor.

In the course of discussions with representatives of iron and steel works, I have been advised that for any particular throughput, a considerable variation in delivery pressure may be required according to furnace conditions.

The use of an axial-flow compressor, in which the whole throughput is supplied to the blast-furnace, does not appear to meet these broad requirements, although such a compressor might

be made suitable by arranging for blow-off under certain conditions. Nevertheless, further information on furnace characteristics and requirements would be very useful.

In Part II (Fig. 8) of the paper, the authors have already indicated these dangers with the straight axial-flow compressor—the surge-line limitation on the one hand and the low adiabatic efficiency on the other hand.

Mr. **R. P. Towndrow** (Messrs. Colvilles, Ltd.): I am speaking with some trepidation as one of two operators entirely surrounded by a sea of engineers. I should like to associate myself with the compliments paid to the authors and to assure them of the very great interest which all operators will take in the information which they have put so clearly before us, and which has gone a long way towards answering some of the questions which we have asked ourselves about these new developments. It may be of interest if I say something about the operator's point of view on blowers, appreciating as I do that these considerations have been taken into account particularly by Mr. Railly, and I think by Mr. Voysey, in the discussions which they have had with operators in preparing their papers.

In the first place, steam plant and blowing plant at most blast-furnaces can never be put down to meet an ideal condition. We have not one blast-furnace and one generator each requiring a particular fixed load at all times. We have usually to put down plant which may have to supply both power and blast on a very varying load and operate units in varying combinations, both boiler units and turbine units, with the general result that all the units are very rarely operating either at maximum load or at their most efficient load. Sometimes they are grievously overloaded and sometimes they are working for very long periods below their optimum-load figures.

With regard to the pressure/volume characteristics which the operator requires, and which always seem to be a source of perplexity to the harassed engineer, the first thing to realize is that the relationship between pressure and volume is not determined by the blower or by the operator, but by the blast-furnace. That is to say, if we operate a blower under constant volume, the pressure at which it will be delivered depends entirely on what the blast-furnace says that it will accept, which may vary, as previous speakers have said, from what we consider the normal operating pressure, and for which we try to design the blower, up to any maximum which the blower is capable of delivering. If we operate the furnace at constant pressure, then conversely the volume which the furnace will accept depends entirely on the furnace and not on any characteristics of the blower, apart from its surging characteristics.

The confusion which has descended on the engineers is largely generated, I think, by the confusion which exists in the blast-furnace-operator's mind, because he finds that to overcome certain undesirable furnace conditions on Monday he wants to keep his furnace to a certain maximum pressure, but when he meets the same conditions on Tuesday he finds that if he keeps, or tries to keep, his furnace to a maximum pressure he does not do any good, and he then decides to work on a maximum volume, which is very confusing to the engineer, and not less so to the operator. The upshot is that blast-furnace operators demand a blower which will not only operate efficiently under normal operating conditions but also give a range of flexibility which is perhaps not equalled in any other industrial plant of similar size.

Passing from that to questions of efficiency, I have already mentioned that most blowing units rarely work either at the best efficiency which they could achieve or at the best efficiency which similar plants could be designed to achieve under different conditions. I should like the authors to give us some idea of the relative efficiencies which we could expect under two conditions. Let us take first of all steam-driven centrifugal blowers, and let us assume a boiler efficiency of 80% (which I think is as high as we can expect over the normal range of ironworks practice) and a blower efficiency of, say, 70% on a centrifugal blower, because it is only on rare occasions that the blower will work at its maximum efficiency of 75%. What the turbine efficiency is I do not know, but I think that Dr. Bowden is in a very good position to say. From that, perhaps the authors could give us the overall thermal efficiency which we might expect at the output flange of the blower, compared with the overall thermal efficiency which we might expect from a gas-turbine blower operating on the 20% efficiency line shown in Fig. 15 of their paper as being halfway between the maximum efficiency line and the surge line.

The other question to which I want to know the answer, but which I strongly suspect cannot be explained without some mathematics beyond my comprehension, is why there is the " wiggle " in the surge line.

AUTHORS' REPLIES

Dr. **A. T. Bowden** (in reply): I shall attempt briefly to deal with some of the points which have been raised. First of all, Mr. Bridge has noted, and rightly, that we must have an alternative fuel. This point is dealt with in the paper, and as far as the gas turbine is concerned the alternative fuel must be oil. Further, if we follow my initial remarks to their logical conclusion, it must for the time being at any rate be a distillate.

The answer is, therefore, that the only alternative fuel to blast-furnace and coke-oven gas which we can consider is oil, and that at the moment, until we have more working experience with some of the heavier oils, it should in our opinion be a distillate.

I much appreciate Mr. Welsh's remarks on the paper, and I have considerable sympathy with his views regarding the immediate use of the bled-axial compressor. On the other hand, the characteristics which one can expect to obtain from a bled-axial compressor can, on the basis of known compressor performance, be fairly thoroughly worked out, and we feel reasonably confident that the characteristics shown in the paper can be realized in practice. It may be, therefore, that far from being the most complicated unit to start the gas turbine, the bled-axial compressor may be the simplest.

I sympathize with Mr. Welsh's outlook, however, and, as I mentioned in the paper, I feel that the most rational solution to the problem is to be obtained through the co-operation of Government- or trade-sponsored bodies in the instaliation of the first plant or plants. It is said in the paper that every sensible engineer likes to have operating experience before he puts up to his board any recommendations with regard to a new type of plant, unless that new type has very outstanding advantages over the old with regard to cost or to efficiency.

That gives me the opportunity to mention a point which I meant to make before with regard to costs. The actual selling price of a steam turbine embraces the experience obtained over years of operation as to servicing requirements. The initial selling cost is not merely a matter of materials and labour, but of long experience, enabling the seller to define his costs very clearly when the plant is sold to the customer. We cannot do that in the case of a gas turbine. Therefore relative costs between gas and steam turbines at the present time do not allow of a true comparison, and this state of affairs must obtain until the shops are tooled up to make the former with equal facility to the latter, and until we have had the experience which will enable us to say what service charges should be associated with them. However, in spite of that we feel that in general the present costs should be less, and in some cases considerably less, than for steam plants of comparable output.

Mr. Thursfield raises the question of oil, and, as noted in the paper, we propose this as the alternative fuel. The axial compressor is ideal for large volumes of air. Mr. Thursfield talks of 80,000 cu. ft./min., and here the axial compressor comes into its own, because inherently it is capable of dealing efficiently with large volumes of air. With regard to the efficiency of the smaller compressor, of 35,000 cu. ft./min., I should expect no substantial drop in the efficiency figures which we have quoted, because, as will be realized, as far as the gas turbine is concerned, while the net output may be 4000 kW., the total output may be 12,000 kW., so that even in a small unit the turbine itself is developing a very large internal output, and therefore the efficiency does not come down to the same extent as does that of a steam turbine with reduction in output.

As for alternative schemes which cut out the stove, we are not, as may be noted, greatly in favour of them at the present stage of development. For one thing, we come into the adventurous field of the behaviour of materials at high temperatures, of the order of 750–850° C., and at the moment we prefer to avoid dealing with such temperatures, on account of the lack of experience and data on the steels which would have to be used.

Mr. Wood mentions compressor efficiencies of 89%, and wonders why we talk of such comparatively poor figures as 83% and 84%. Figures of 83% and 84% are, as mentioned in the paper, reasonably attainable with commercial methods. Compressor blades are notoriously dear to make, and if one goes to the full limits of desirable aerodynamical shape the difficulties are such that the cost becomes prohibitive. Even so, I may say that I find considerable difficulty in believing his figure of 89%, obtained in Peru, and I should like to know the basis on which that efficiency has been calculated. I would say that 89% is much higher than we should expect with normal production methods at the present day, in spite of making use of the aerodynamical advances of which we are fully aware. We feel that 83–84% is reasonably attainable, and that 86% is quite possible, though with a little difficulty ; but 89% is from our point of view, and I think from that of most other commercial compressor builders, a little out of the picture.

Mr. Wood also refers to our change of front in regard to the centrifugal and bled-axial compressors. As I have already mentioned, this is largely because of the amount of work which we have done, admittedly on paper so far, but it has been checked and rechecked, and we feel that the bled-axial type can more cheaply and more efficiently meet normal blowing requirements. We have not obtained any data from, nor been influenced by, the sources to which he refers.

The starting motor to which Mr. Wood refers is dealt with in the paper, and the figure of 5% which he quotes is the one which we give there

In reply to Mr. Calderwood, the efficiencies quoted in Figs. 3 and 6 of the paper do embrace the losses in the exchanger, the pipe-lines, and the

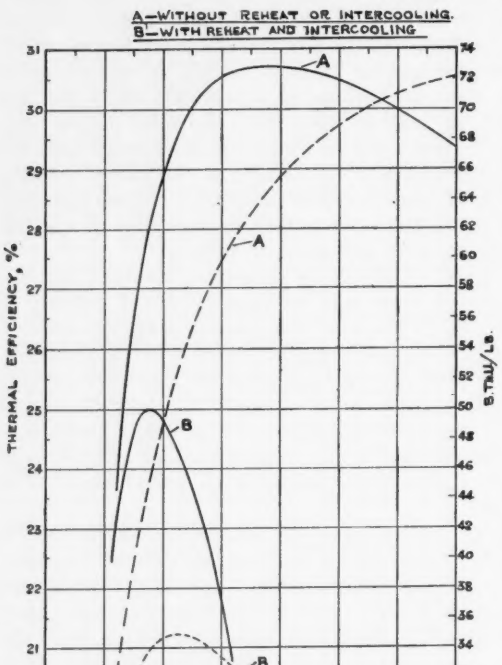


FIG. A—Combined effect of reheat and intercooling on the overall efficiency (full lines) and on the specific output (broken lines), employing a 75% effective heat exchanger

combustion chamber. The combined effects of intercooling and reheat are shown in Fig. A.

On the question of the variation of efficiency with load, he asks whether it would not be advisable to consider the use of the closed or the mixed cycle in order to obtain better partial-load efficiencies. I have dealt with part-load efficiencies to a small extent in the paper and have shown that the simple turbine was penalized on part load, but that the two-line open-cycle type suffered very little in comparison with, say, the steam turbine in part-load operation. Personally, I should be prepared to sacrifice part-load efficiency in an open cycle rather than go in for the adventure of an air boiler which is associated with the closed or the mixed cycle. There is little doubt that in the long run the closed cycle, and possibly the mixed cycle, must have their place in gas-turbine developments, particularly for large outputs, but they do lose the inherent simplicity of the simple open cycle, and for the outputs which we have been discussing this afternoon I feel that there is no need to depart from that inherent simplicity for the sake of possible increased part-load operating efficiencies.

The gas must be clean, as we have noted in the paper, and the degree of cleanliness obtained by normal wet or wet and dry cleaning operations seems to be good enough for what we want.

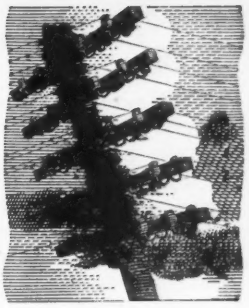
Mr. Baumann, whose comments are much appreciated, refers to the control problems of the bled-axial machine. As we have shown in the paper, the control problems are simple within the normal operating range of the turbine, and it is only when it is necessary to take care of special operating conditions such as snorting and blowing-in that the control problem arises. These latter are, however, infrequent operations, and can, we feel, be dealt with adequately by a relatively simple and straightforward control system.

Mr. Baumann suggests a gas turbine driving a centrifugal compressor, and I sympathize with his outlook ; but I feel that unless it can be shown that what we claim for the bled-axial is not likely to be obtained, we should take advantage of its simplicity, efficiency, and relative cheapness, coupled up to a gas turbine, if it is at all possible. I think that it is possible, and I think that it is the thing to go for at this stage, unless it can be shown that the calculations we have placed before you are not in fact likely to be realized.

In reply to Mr. Towndrow, I should like to say a word on the question of the efficiencies to which he refers. If we take the boiler efficiency of the steam-driven plant as 80%, and that of the blower as 70%, we would normally get at the turbine shaft, at the coupling to the blower, an efficiency of 21–22% with the plant normally installed in steelworks at the present day, which would give us an overall efficiency of some 15% for the air delivered to the blast-furnace. The gas turbine, on a conservative basis, should have a shaft efficiency of 21–22%. If we take a blower efficiency of the order of 80% over its normal working range, the overall efficiency is of the order of 17%.

Mr. **R. G. Voysey** (in reply) : The only question which seems to remain unanswered is why there should be a squiggle in the surge line. The original characteristic belonging to the compressor proper had a slight squiggle, and if you imagine that an amount is bled off by a turbine, which behaves much the same as a simple orifice, that will follow a smooth curve. The difference between the amount taken by the turbine and the amount dealt with by the compressor is the amount that goes to the blast-furnace, and so the squiggle is accentuated, because whereas the original squiggle was only 5%, if you take away two-thirds you must expect that effect to go up three times and therefore to have a 15% deformation of the curve. That explains the rather odd shape of the surge characteristic.

N E W S



ANNOUNCEMENTS AND NEWS OF SCIENCE AND INDUSTRY

THE IRON AND STEEL INSTITUTE

Autumn Meeting, 1947

The Autumn Meeting of the Iron and Steel Institute will be held on Wednesday and Thursday, 12th and 13th November, 1947. The sessions for the whole day on Wednesday, 12th, will be held at the Institution of Civil Engineers, Gt. George Street, London, S.W.1, whilst the second session, on the morning only of Thursday, 13th, will be held at the offices of the Institute, 4, Grosvenor Gardens, London, S.W.1. A buffet luncheon will be given in the Library of the Central Hall, Westminster, on Wednesday, 12th November.

The following provisional arrangements for the programme have been made :

Wednesday morning—presentation and discussion of papers on Hydrogen in Steel, and Brittle Fracture in Mild Steel.

Wednesday afternoon—presentation and discussion of papers on Producer Gas, and Pyrometry.

Thursday morning—presentation and discussion of papers on Fluidity of Steel, and Steelmaking Practice.

A more detailed programme will be given in the next issue of the Journal.

Joint Meeting in Manchester

At the invitation of the Manchester Metallurgical Society, a joint meeting of that Society with the Iron and Steel Institute and the Institute of Metals will be held in the Engineers' Club, Albert Square, Manchester, on Wednesday, 26th November, 1947, commencing at 6.30 P.M.

Dr. L. C. Bannister, President of the Manchester Metallurgical Society for the Session 1947–48, will be in the Chair. Mr. E. W. Colbeck (of Messrs. Hadfields, Ltd., Sheffield) will read a paper on "*Some Metallurgical Problems in the Field of Atomic Energy.*"

Joint Library

In connection with the meeting of the Iron and Steel Engineers Group, on Wednesday, 26th November, 1947, the Reference Library will be closed for the whole of that day.

Changes of Address

Members are advised that changes of address, for the despatch of copies of the monthly *Journal*, should reach the offices of the Institute by the 12th day of each month. Failure to notify the new address in good time will mean that the *Journal* will be sent to the last-recorded address.

The Editor invites members to send him details of changes of occupation or other items of personal news which may be of interest to other members.

Czechoslovak Association of Mining and Metallurgical Engineers

By arrangement between the Council of the Association and the Council of the Iron and Steel Institute, Members of the Association of Czechoslovak Mining and Metallurgical Engineers will be entitled to become Members of the Iron and Steel Institute at the following reduced fees :

Ordinary Members—£2 12*s.* 6*d.* a year instead of £3 3*s.* 0*d.* (entrance fee £2 2*s.* 0*d.*).

Associate Members—£1 1*s.* 0*d.* a year instead of £1 5*s.* 0*d.* (no entrance fee).

Whilst the Institute will be sending to Czechoslovakia copies of the monthly *Journal*, it will receive, in exchange, copies of the two periodicals *Baňský obzor* and *Hutnické listy*.

The Association has a membership of about 600. The Chairman is Professor Ing. Dr. Jinřich Šádek, Assistant General Director of the Vítkovice Steel Works, and the Vice-Chairman is Ing. Svatopluk Rada, General Director of the Czechoslovak Mines National Corporation. The Offices of the Association are in Prague but its activities are split into local sections which specialize in mining and metallurgy. Metallurgical Sections are in Prague, Moravská Ostrava and Plzeň.

Staff Biography No. 9

Dr. DORA STEINER was born in Stuttgart in 1909. Having matriculated in 1928, she studied chemistry at the Technical University there, and also at Berlin, taking metallurgy, physics, mineralogy and the fundamentals of mechanical engineering as subsidiary subjects. After graduating as Diplom-Ingenieur in 1932, she gained her doctorate in 1934 with a thesis on "Chemical Reactions between Colloids," which was published in *Kolloidzeitschrift*.

In 1935 she came to London and for 18 months worked at University College under Professor Freundlich, studying the properties of colloidal solutions of silver and copper. The results appeared in the *Journal of the Chemical Society*.

For some months in 1937, Dr. Steiner worked in the library of the Research Association of British Rubber Manufacturers in order to gain experience in technical librarianship and abstracting.

In January, 1938, she was engaged as an editorial assistant by the Institute of Metals, and in June, 1939, she transferred to the Iron and Steel Institute. At first she was mainly engaged in general editorial work but as time went on her activities centred more and more round the Institute's Translation Service, of which she has been in charge during recent years. The large majority of the translations from German and French issued by the Institute were prepared by her, and she undertook the editing of all the German, French, and Russian translations, having acquired some Russian towards this end.

On the 1st October, 1947, Dr. Steiner will take up a new position with the British Oxygen Co., Ltd., as Assistant Intelligence Officer. She will take with her the best wishes of all her colleagues and friends.

NEWS OF MEMBERS

Appointments

▸ Dr. J. R. RAIT has left the service of Messrs. William Jessop and Sons, Ltd., and has taken up an appointment with Messrs. Hadfields, Ltd., Sheffield.

▸ Mr. E. T. BARRON, Chief Metallurgical Engineer of the Carnegie-Illinois Steel Corporation, is retiring and will be replaced by Mr. L. J. Rohl.

▸ Dr. K. BALAJIVA has taken up an appointment in the Government Mines Department, Bangkok, Siam.

▸ Mr. B. CHETWYND TALBOT has been appointed deputy Chairman and General Manager of the South Durham Steel and Iron and Cargo Fleet Companies.

▸ Mr. C. H. CUNNIFFE, M.B.E. has been appointed a Director of Eaglebush Tinplate Works, Ltd., Neath.

▸ Dr. ING. M. BALICKI has been appointed Assistant Professor of Metallurgical Engineering in the Polytechnic Institute of Brooklyn, New York.

▸ Mr. WILLIAM B. BROOKS has left the Alloys Development Company, and has established a consulting practice specializing in stainless and alloy steels.

▸ Mr. E. H. ARMITAGE has resigned from the Chairmanship of the Hoffmann Manufacturing Co., Ltd., but retains his seat on the Board.

▸ Sir CLIFFORD PATERSON, O.B.E., F.R.S., has been appointed Chairman of the General Council of the British Standards Institution, in succession to Sir William J. Larke, K.B.E.

▸ Mr. J. WOOLMAN has recently been appointed a member of Council of the Production Engineering Research Association.

▸ Dr. F. JOHNSON is retiring from his position as Head of the Department of Metallurgy, Birmingham Central Technical College.

▸ Mr. B. H. DELONG has been appointed Vice-President and Technical Director of the Carpenter Steel Co., Reading, Pa., U.S.A.

▸ Mr. G. V. LUERSSEN has been appointed Chief Metallurgist of the Carpenter Steel Co., U.S.A.

▸ Mr. S. T. JAZWINSKI, Vice-President of the Sheffield Iron and Steel Company, Alabama, U.S.A., is to undertake the management of the foundry of George King Co., now a part of the Sheffield Iron and Steel Company's organization.

▸ Dr. D. W. DAVISON has been appointed Senior Research Officer, Physical Metallurgy Section, of the Commonwealth of Australia Council for Scientific and Industrial Research, Melbourne.

▸ Mr. T. HENRY TURNER has been given full membership of the Institution of Mechanical Engineers.

▸ Monsieur JEAN LATOURTE has been appointed Directeur Général adjoint Société Métallurgique de Knutange.

▸ Mr. C. R. HOOK has been elected President and Chairman of the executive committee of the American Rolling Mill Company.

▸ MR. C. VERITY has been elected Chairman of the finance committee of the American Rolling Mill Company.

▸ Mr. P. M. COOKE is joining the staff of B.I.S.R.A. under the Mechanical Working Division.

▸ Dr. L. K. JENICEK is now Research Director of the United Steelworks National Corporation, the original title, "Czechoslovak National Steel Corporation," no longer being used. Dr. Jenicek is also Reader in Physical Metallurgy at the Czechoslovak Mining and Metallurgical Institute, Ostrava.

▸ Mr. G. B. EVANS has been appointed Senior Metallurgist to Airspeed, Ltd., Portsmouth.

▸ Mr. S. G. HENDERSON has left the Fullers' Earth Union, Ltd., to take up an appointment with Messrs. Colin Stewart, Ltd., Cheshire.

▸ Mr. J. W. KINNEAR, jun., has been appointed executive vice-president of the Firth Sterling Steel and Carbide Corporation, McKeesport, Pa., U.S.A.

▸ Mr. S. THOMSON and Mr. J. GIBSON have been appointed executive directors of Messrs. Colvilles, Ltd.

▸ Mr. L. BRADWELL has joined the Attock Oil Co., Ltd., Rawalpindi, India.

▸ Mr. G. HOYLE is now on the staff of the British Iron and Steel Research Association.

▸ Mr. T. CROSTA has joined the metallurgical staff of the Algoma Steel Corporation, Ontario, Canada.

▸ Mr. DEWI M. LEWIS has joined the staff of the British Iron and Steel Research Association and will work in the Physics Laboratory at Battersea.

▸ Mr. M. TIPLER has taken up an appointment at the National Physical Laboratory, Teddington.

▸ Mr. R. ELLIS has joined Messrs. Rediffusion, Ltd., London.

▸ Dr. P. VAJRAGUPTA is leaving this country to join the Government Mines Department, Bangkok, Siam.

▸ Mr. A. G. BLOOMER has been appointed Works Manager of Messrs. Whiteley Read Engineers, Ltd., Nottingham.

▸ Mr. H. W. GRAHAM has been made a Vice-President of the Jones and Laughlen Steel Corporation, Pittsburgh, Pa., U.S.A.

▸ Mr. JOHN WALKER has been appointed a Director of the Morgan Crucible Co., Ltd.

Awards

The President and Members of Council offer their congratulations to:

Dr. H. J. GOUGH, C.B., M.B.E., D.Sc., F.R.S., who has been awarded the Medal of Freedom with Silver Palm by the United States of America. At the presentation ceremony, on 21st August, 1947, the following citation was read:

"Doctor Herbert John Gough, United Kingdom, during the period of active hostilities in World War II, performed exceptionally meritorious service in the field of scientific research and development. An engineer-scientist and leading authority on strength of materials, as Director-General of Scientific Research and Development in the Ministry of Supply, he was responsible for much of the British program for development of ground-force weapons, actively co-operating in establishing and supporting throughout the war, arrangements for the Anglo-American exchange of important relevant information."

Obituary

The Council regret to record the deaths of:

Mr. GEORGE LEWIS, Director of Thos. Perry and Son, Bilston, on 7th August, at the age of 77.

Mr. JOHN A. HORNE, Assistant Metallurgist, Newton, Chambers & Co., Sheffield, on 16th August, aged 21.

Mr. ANDREW JOLLIE, General Works Manager, Steel, Peech and Tozer, Sheffield, on 23rd August.

CONTRIBUTORS TO THE JOURNAL

R. Jackson, Ph.D., M.Sc.—Radiologist and Metallurgist in the Research Department of Messrs. Hadfields, Ltd. He graduated at Manchester University in 1930 and later did research there in X-ray crystallography for the M.Sc. degree. After holding a teaching appointment he was engaged as a research assistant at Sheffield University, applying physical methods to metallurgical problems, and received a Ph.D. degree in 1939.

At the outbreak of war Dr. Jackson was appointed radiologist to Messrs. Hadfields and took charge of the examination of all aircraft steel castings made in the foundry. Later he entered a wider sphere of research activity covering general problems of production.

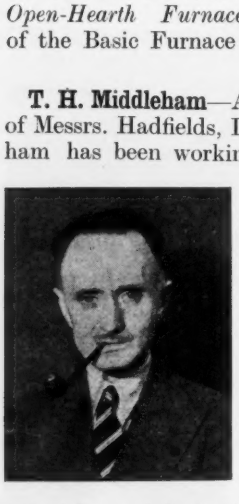
He is a member of the Iron and Steel Institute, won the Williams Prize in 1945, and represents the Institute on the Institute of Physics Committee for the Terminology of Casting Defects. He has written papers on X-ray and Electron Diffraction, Heat Flow in Steel, Radiography, Special Steels and Non-Destructive Testing.

Robert C. Baker—Melting Shop Manager at Messrs. John Baker and Bessemer, Ltd., Mr. Baker was educated at Sheffield University, where he obtained his Associateship in Ferrous Metallurgy in 1937. He trained for a year in the Research Department and Melting Shop of the English Steel Corporation, Ltd., and then took up the post of Assistant Melting Shop Manager

in the family firm, Messrs. John Baker and Bessemer, Ltd., at the Kilnhurst Steel Works. In 1938 he was promoted to Melting Shop Manager, becoming responsible for steel production, acid and basic open hearth, and the maintenance, extension, and improvement of the steel-producing plant. He was, at the same time, in charge of the firm's Chemical and Metallurgical Laboratories.

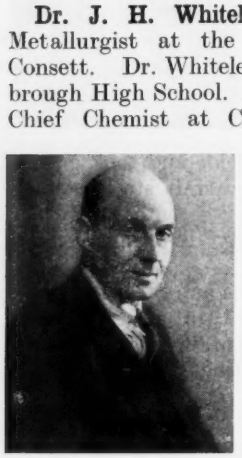
Mr. Baker has published a paper entitled "*Monolithic Dolomite Doors and Wing Walls for Open-Hearth Furnaces*" for the 2nd Report of the Basic Furnace Linings Committee.

T. H. Middleham—Assistant Research Manager of Messrs. Hadfields, Ltd., Sheffield. Mr. Middleham has been working for more than 20 years in the Research Department of this company, only breaking this long period of service during 1933 to 1934, when he was seconded to the Chemical Research Laboratory of the Department of Scientific and Industrial Research. During this time he was engaged in conducting a co-operative research on materials used for high-pressure chemical plant.



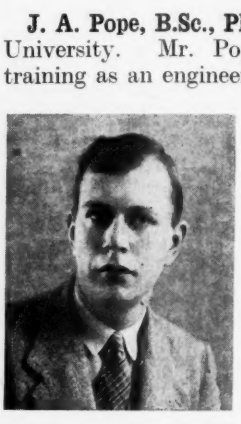
Mr. Middleham has published several papers on various metallurgical subjects.

Dr. J. H. Whiteley, D.Sc., F.R.I.C.—Chief Metallurgist at the Consett Iron Co., Ltd., Consett. Dr. Whiteley was educated at Middlesbrough High School. After being assistant to the Chief Chemist at Cargo Fleet Steelworks he became Chief Chemist at the South Durham Steel and Iron Company's Malleable Works, in 1905. He held this position until 1928, when he was appointed to his present position.



Dr. Whiteley has presented many papers to the Iron and Steel Institute and the Cleveland Institution of Engineers, and his subjects have been in particular the acid hearth and slag and inclusions in steel. In 1917 he was awarded, jointly with Mr. A. F. Hallimond, a grant from the Andrew Carnegie Research Fund, for a joint research on the chemical detection of strain in iron and steel, and in the same year he received the Carnegie Gold Medal for his paper entitled "*The Eggertz Test for Combined Carbon in Steel.*" In 1943 Dr. Whiteley received the Bessemer Gold Medal for his contributions to the knowledge of iron and steel manufacture. He has been a member of various Joint Research Committees of the Iron and Steel Institute.

J. A. Pope, B.Sc., Ph.D.—Lecturer at Sheffield University. Mr. Pope received his practical training as an engineering apprentice at Boulton and Paul Ltd., Norwich, from 1930 to 1935. During this period he obtained National Certificates in Mechanical Engineering and finally a Whitworth Scholarship in 1935.



From 1935 to 1938 he was a student at King's College, London, obtaining a Diploma with 1st Class Honours in 1937. During 1937 to 1938 he carried out research work on "The Pressure Waves in Suddenly Released Gases."

In 1938 Mr. Pope was appointed Lecturer in Mechanical Engineering at the College of Technology, Belfast. He obtained the B.Sc. of London University in 1939, and was awarded the degree of Ph.D. at Queen's University, Belfast, in 1942, for research into "The Change of Mechanical Properties of Mild Steel During Repeated Impact." He became Assistant Lecturer in Engineering at Manchester University in 1944, and Lecturer at Sheffield University in 1945.

R. J. Sarjant, O.B.E., D.Sc.—Professor of Fuel Technology in the University of Sheffield.

D. Knowles, Assoc.Met., F.I.M.—Metallurgist in the Research Department of Messrs. Hadfields, Ltd., Sheffield.

H. R. Schubert, Ph.D.—Assistant in the Department of German, University of Reading.

W. I. Pumphrey, M.Sc.—Leader of the Aluminium Welding Research Team of the Aluminium Development Association at the University of Birmingham.

Full biographical notes on Dr. Sarjant, Dr. Schubert, and Mr. Knowles were given in the News Section of the April issue of the *Journal* (pages 625–626), and on Mr. Pumphrey in the May issue (page 125).

EDUCATION

"Science Survey"—Metallurgy

A programme devoted to Metallurgy was given in the Home Service of the B.B.C., from 7.30 to 8.0 P.M. on Wednesday, 25th June, in which an attempt was made to present a brief résumé of some contemporary metallurgical research themes for the benefit of the average scientifically inclined listener.

The programme was devised by Dr. Norman Stuart in collaboration with Dr. Archibald Clow, the B.B.C. Director of the "Science Survey" series of weekly talks, and those taking part were Dr. N. P. Allen, Mr. A. J. Murphy, and Mr. D. A. Oliver.

The speakers were announced as "Specialists in the science of metals . . . metallurgists," and were then introduced in turn by Dr. Stuart, who first called upon Dr. Allen to say something about the nature of the metallic state and the ideas at present held on the nature of alloys. Mr. Murphy then illustrated these views with a brief description of the Duralumin alloys and the phenomenon of "age hardening," following up with a short comment on the strength of metals in relation to their crystalline properties. The importance of aluminium–magnesium alloys for aeronautical construction was illustrated, and the subject of elasticity also touched on. Mr. Oliver concluded the broadcast with a survey of the scientific problems which confront the steelmaker when producing large steel forgings of the highest-quality steel, and illustrated his theme by reference to the special steels required for use in aeroplane jet engines and ships' turbines, and the particular conditions therein encountered.

It is hoped that a revised version of the script of this broadcast will be prepared for publication in *The Listener* or elsewhere.

IRON AND STEEL ENGINEERS GROUP

The **Fifth Meeting** of the Iron and Steel Engineers Group will be held at 4, Grosvenor Gardens, London, S.W.1, on Wednesday, 26th November, 1947.

At the morning session, Mr. W. W. Franklin and Mr. P. F. Grove will present for discussion a paper on "*The Mechanical and Electrical Features of the Principal Hot-Rolling-Mill Auxiliaries.*"

After a Buffet Luncheon, to be held in the Library, the afternoon session will be devoted to the presentation and discussion of a paper on "*The Development of Modern Small Rolling Mills*" by Mr. G. A. Phipps. The two papers for this meeting will be published in the October issue of the *Journal*.

BRITISH IRON AND STEEL RESEARCH ASSOCIATION

Corrosion Research

The address by Dr. J. C. Hudson, to be presented in October at the Conference on Corrosion, in Paris, will be published in English, complete with illustrations, in the November issue of *Iron and Steel*.

Critical Reviews of Literature

The Association hopes to publish from time to time, in the B.I.S.R.A. Section of the *Journal*, critical reviews and surveys of literature on subjects relative to the work of its committees. The first two, prepared by Dr. N. P. Allen for the Alloys Steels Research Committee, are not in a form suitable for publication, but duplicated typed copies may be obtained on application to the Association at 11, Park Lane, London, W.1.

The titles and references are as follows :

A Preliminary Survey of the Literature dealing with the Effects of Alloying Elements on the Mechanical Properties of Ferrite. (Ref. G.5/46).

A Preliminary Survey of the Literature dealing with the Effects of Alloying Elements on the Properties of Ferrite—Section II. Iron-Nitrogen Alloys. (Ref. MG/A/9/46).

THE INSTITUTE OF METALS

Symposium on Internal Stresses

The Institute of Metals, in association with the Faraday Society, the Institute of Physics, the Institution of Mechanical Engineers, the Iron and Steel Institute, the Physical Society, and the Royal Aeronautical Society, has arranged a "*Symposium on Internal Stresses in Metals and Alloys*," which will be held on Wednesday and Thursday, 15th and 16th October, 1947, at the Institution of Mechanical Engineers, London, S.W.1.

The object of the meeting is to bring together engineers, metallurgists, and physicists to discuss problems of common interest. Of the many papers contributed for discussion several report the results of original research.

The meeting and discussions will be open (free) to all who may be interested, but attendance at buffet luncheons and the conversazione will be limited to members of the participating societies. A circular, with reply form attached, is being distributed to the members of these societies.

The presentation of papers and discussion is being divided into three sessions: On Wednesday morning, after the Introductory Address by Colonel P. G. J. Gueterbock, President of the Institute of Metals, Session I, on *Measurement of Internal Stresses*, will be introduced by the

Rapporteur, Mr. D. A. Oliver. Dr. C. Sykes, F.R.S., will be the Chairman. In the afternoon, under the Chairmanship of Professor Leslie Aitchison, Session II, on the *Origin, Control and Removal of Internal Stresses*, will be introduced by Dr. H. O'Neill, Rapporteur. The final Session III, on *Effects Associated with Internal Stresses*, will be held on Thursday, Dr. Maurice Cook and Dr. H. J. Gough, F.R.S., being in the Chair, with Dr. A. G. Quarrell and Dr. N. P. Allen as Rapporteurs.

NEWS OF SCIENCE AND INDUSTRY

I.L.O. Industrial Committees for the Iron and Steel and Metal Trades

Two of the seven international committees set up by the Governing Body of the International Labour Office, to deal with conditions in major industries, recently held their second sessions. The Iron and Steel Committee met on 20th August, and the Metal Trades Committee on 2nd September; both meetings were held in the Parliament Building, Stockholm.

Each of the seven Committees (which cover inland transport, textiles, coalmining, iron and steel production, the metal trades, petroleum production and refining, and building, civil engineering, and public works) is of a tripartite character, consisting of representatives of the Governments, employers, and workers of the countries most directly concerned with the respective industries. The general terms of reference of each Committee are to consider the present social problems of its industry and future international co-operation on social policy and its economic foundations in the industry. The Committees have authority to recommend action on the part of the employers' and workers' organizations concerned in the various countries, as well as to recommend action by the I.L.O.

The Iron and Steel Committee held its first session in Cleveland, Ohio, in April of last year, and the Metal Trades Committee in Toledo, Ohio, in the following month. At the recent sessions they received reports on developments since those meetings, and also considered the following questions:

(1) Regularization of production and employment at a high level;

(2) Minimum income security (annual and other wage systems designed to provide assured earnings); and

(3) Labour-management co-operation.

The following members of the Institute have been nominated to attend the meetings of the Iron and Steel Committee as representatives and advisers from the United Kingdom:

Colonel J. M. BEVAN (Chairman, Briton Ferry Steel Co., Ltd.)

Mr. R. MATHER (Chairman, Skinningrove Iron Co., Ltd.)

Sir LEWIS JONES (Secretary, South Wales Siemens Association.)

Foundry Congress at Aachen

Das Giesserei-Institut held its first Foundry Congress since the war at the Technischen Hochschule, Aachen, during the three days, 23rd to 25th July, 1947. For ten years prior to 1938 under the inspiration of Professor Piwowarsky, the annual congress of the Giesserei-Institut was an international event of consequence to all concerned with the various branches of foundry technology, and the fact that this, the eleventh congress, was not only possible but also was such a great success was obviously a source of much satisfaction to Professor Piwowarsky and to those others who had had the privilege of attending on the earlier occasions. The Congress was attended by about three hundred technical men from all parts of Germany, as well as by a number of foreign delegates, those from this country being Mr. E. H. Salinger, Managing Director of the Iron Foundry of Messrs. Truvox Engineering Co., Ltd., Mr. H. Morrogh of the B.C.I.R.A., and Dr. W. C. Newell of B.I.S.R.A. Mr. A. Whiteley, of the Control Commission in Germany, was also present.

Professor Piwowarsky opened the Congress by giving a paper detailing the various stages in the development of modern high-quality cast iron. This was followed by a paper by Mr. C. Adey on the important subject of spheroidizing the graphite present in cast iron in order to improve its resilience. Other papers dealt with the properties of cast steel, foundry mechanization, cast lead-bronze, aluminium-silicon alloys, powder metallurgy, hot-blast cupolas, and core-binding materials. Altogether there was a very full and varied programme which occupied far more than the scheduled time and which was followed by keen discussion. There is no doubt that the Congress served a most useful purpose, and it is hoped that it will be followed by many more in the years to come.

W. C. NEWELL

Cracking of Welded Steel Gas Mains

A Committee of the British Welding Research Association, on which there are representatives from the Gas Research Board and the British Iron and Steel Research Association in addition to members of industrial and university research laboratories, has been undertaking the investigation of the causes of stress-corrosion

cracking in welded mild-steel pipes exposed to crude coal gas. The Committee published a statement on the problem some twelve months ago and now presents the following summary of the present position :

(1) The cracking of welded gas plant is a particular case of the general phenomenon of intercrystalline cracking.

(2) The fact that welded steel gas plant is subject to stress-corrosion cracking indicates the combined presence of a state of stress in the steel and of a specific corrosive liquid medium.

(3) There is so far no evidence to show that differences in welding technique have influenced the incidence of cracking.

(4) In certain other cases of intercrystalline cracking met with in industry (which may or may not be analogous to the cracking of welded steel gas mains), considerable benefit can be obtained by thermal stress-relieving processes, or by shot-blasting. It is possible that the solution to the problem of the cracking of welded gas mains may be found in one or other of these treatments, but it will probably be important to specify fairly accurately the method of procedure, since cases are not unknown where incorrect treatments of this kind have made matters worse.

(5) Observations have suggested that the oxidation products of ammoniacal liquor constituents, such as ammonium thiocyanate, are an important factor in producing corrosion of this type.

(6) Field experiments, covering various types of steels and other factors, have been instituted in gasworks where failure has already been experienced. Many data have been obtained from accelerated laboratory tests which have been used as a guide in planning the field tests but it must be appreciated that results from such tests may not be precisely parallel with those from actual gasworks.

(7) It is hoped that the field experiments may, particularly in conjunction with laboratory work still proceeding, give further information on the chemistry and the mechanism of the cracking, so that the conditions for establishing immunity will be more fully understood.

FOURTH EMPIRE MINING AND METALLURGICAL CONGRESS

Notice of Postponement

The President (Sir Henry T. Tizard, K.C.B., A.F.C.) and Organizing Committee of the Fourth Empire Mining and Metallurgical Congress have found it advisable to postpone the Congress, which was to have been held in London in 1948, until June or July, 1949.

This decision was reached with great reluctance, but improvement in travelling and other facilities has been less than was anticipated, and the present outlook would seem to indicate the desirability of postponement until 1949.

The Committee trust that those who had planned to attend the Congress will not be greatly inconvenienced by this postponement, which should however facilitate the arrangement of a more comprehensive programme.

DIARY

23–26, Sept.
INSTITUTE OF METALS—Autumn Meeting, Offices of the Institution of Engineers and Shipbuilders in Scotland, Glasgow.

25–26, Sept.
INSTITUTE OF PHYSICS and PHYSICAL SOCIETY —Electron Jubilee Celebrations, London.

6–10, Oct.
Conference on the Corrosion of Metals, Paris.

8, Oct.–10, Dec.
NORTHAMPTON POLYTECHNIC—Course of Lectures on Refractories, St. John Street, E.C.1.

12–13, Nov.
IRON AND STEEL INSTITUTE—Autumn Meeting, Institution of Civil Engineers, Gt. George St., S.W.1, and 4, Grosvenor Gardens, S.W.1.

15–16, Oct.
INSTITUTE OF WELDING—Presidential Address, Institution of Civil Engineers.

26, Nov.
IRON AND STEEL ENGINEERS GROUP—Fifth Meeting, on "*Rolling-Mill Auxiliaries*," 4, Grosvenor Gardens, S.W.1.

26, Nov.
MANCHESTER METALLURGICAL SOCIETY — Joint Meeting with Iron and Steel Institute and Institute of Metals, "*Some Metallurgical Problems in the Field of Atomic Energy*," by E. W. Colbeck, Engineers' Club, Albert Square, Manchester.

CORRIGENDA

Second Hatfield Memorial Lecture

The following corrections should be noted to the paper entitled "*Steels for Use at Elevated Temperatures*," by C. Sykes, D.Sc., Ph.D., F.Inst.P., F.R.S., which appeared in the July issue of the *Journal* :

Page 332, Table IX (*e*) : *delete* " 800."

Page 332, Table IX (*f*), the inscription should read : " Tensile Properties of F.C.B. Creep Test-Piece (0·391 in. dia.), after 6615 hr. at 650° C. and a Stress of 4 tons/sq. in."

Page 333, Table IX (*g*), heading to left-hand column should read " Exposure Temperature, °C."

Page 333, Table IX (*h*), the inscription should read : " Creep Stress for Indicated Strain in 1000 hr. (tons/sq. in.)."

Page 334, column 2, last line : *Delete* " invariably."

Page 337, inscription to Fig. 36 : For " 750° C.," read " 650° C."

Page 340, column 1, line 26 : For " pp. 543–455," read " pp. 453–455."

Page 340, column 2, line 54 : For " high steel," read " high-speed steel."

TRANSLATION SERVICE

Since the announcement made in the August issue of the *Journal* (*see* page 570), further translations have been put in hand and the following are now available or in course of preparation.

TRANSLATION AVAILABLE

No. 319 (German). F. WESEMANN : "The Relationship between Heat and Power Economy and Production Conditions in the Larger Iron and Steel Works." (*Stahl und Eisen*, 1947, vol. 66–67, Jan. 30, pp. 35–42).

TRANSLATIONS IN COURSE OF PREPARATION

(German). W. BADING : " The Development of Basic Converter Practice " (*Stahl und Eisen*, 1947, vol. 66–67, Apr. 24, pp. 137–149 ; May 22, pp. 180–186 ; June 19, pp. 212–223).

(German). G. BULLE : " The Production of Pig Iron with Low-Iron Burdens." (*Stahl und Eisen*, 1947, vol. 66–67, Mar. 27, pp. 69–78).

(German). W. EILENDER and W. ROESER : "Metallurgical Investigations on the Working of the Basic Bessemer Process with Oxygen-Enriched Blast." (*Stahl und Eisen*, 1939, vol. 59, Sept. 21, pp. 1057–1067).

(German). A. GELEJI : " The Theoretical Problems Involved in the Design of Rolling-Mill Stands." (*Royal Hungarian Palatine-Joseph Universtity, Publications of the Department of Mining and Metallurgy*, 1941, vol. 13, pp. 224–242).

(German). A. GELEJI : " Calculating the Forces Arising and the Power Requirements in the Mannesmann Tube-Rolling Process." (*Royal Hungarian Palatine-Joseph University, Publications of the Department of Mining and Metallurgy*, 1941, vol. 13, pp. 208–223).

(French) R. GIRSCHIG : " A New Micro-Hardness-Tester. ' The L.C. Micro-sclerometer.' " (*Revue de Métallurgie*, 1946, vol. 43, Mar.–Apr., pp. 95–112).

(Russian). M. A. GLINKOV and V. C. KOCHO : " On the Question of Heat Transfer in a Molten Bath by Gas Bubbles." (*Bulletin de l'Académie des Sciences de l'URSS Classes des Sciences Techniques*, 1946, No. 10, pp. 1463–1472).

(German). G. F. HÜTTIG : "The Theoretical Bases of Sintering Processes in Powders." (*Metallwirtschaft*, 1944, vol. 23, Oct. 20, pp. 367–372).

(German). W. KOENNECKE : " New Nozzle Designs for Low and Medium Reynolds Numbers." (*Forschungen aus dem Gebiete des Ingenieurwesens*, 1938, vol. 9, pp. 109–125).

(French). P. LAURENT : " Contribution to the Theory of the Plasticity of Metals and Alloys." (*Revue de Métallurgie*, 1945, vol. 42, Mar., pp. 72–92 ; Apr., pp. 125–132 ; May, pp. 156–167 ; June, pp. 194–203 ; July, pp. 230–239).

(German). K. RUMMEL : "The Gas Balance in Iron and Steel Works " (*Stahl und Eisen*, 1947 vol. 66–67, Jan 2, pp. 19–23).

(German). E. SIEBEL : " Present Knowledge of the Mechanics of Wire Drawing." (*Stahl und Eisen*, 1947, vol. 66–67, May 22, pp. 171–180).

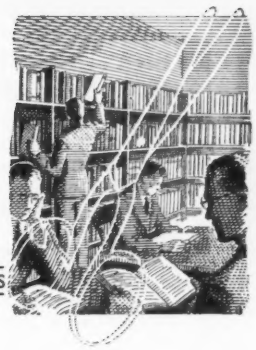
(Swedish). E. SUNSTRÖM : " The Development of the Electric Steel Furnace during the World War 1939–1945." (*Jernkontorets Annaler*, 1946, vol. 130, No. 10, pp. 477–549).

(Russian). F. F. VITMAN, N. N. DAVIDENKOV and N. A. ZLATIN : " Cold-Brittleness of Steel in Torsion." (*Journal of Experimental and Theoretical Physics, U.S.S.R.*, 1940, vol. 10, Nos. 9–10, pp. 1137–1145).

CHARGES FOR COPIES OF TRANSLATIONS.—For the above Translations a charge will be made of 10*s*. for the first copy and 5*s*. for each additional copy of the same translation. Requests should be accompanied by a remittance. The above translations are not available on loan from the Joint Library.

TRANSLATIONS PREPARED AT MEMBERS' REQUEST. — Members requiring translations of foreign papers are invited to communicate with the Secretary, who will ascertain whether the translations can be prepared for inclusion in the series.

ABSTRACTS OF CURRENT LITERATURE *and* BOOK NOTICES



IRON AND STEEL MANUFACTURE AND RELATED SUBJECTS

CONTENTS

	PAGE		PAGE
Ores—Mining and Treatment	139	Welding and Flame-Cutting	148
Temperature Measurement and Control	140	Cleaning and Pickling	149
Blast-Furnace Practice and the Production of Pig Iron	140	Protective Coatings	150
Production of Steel	141	Properties and Tests	152
Foundry Practice	142	Metallography	154
Heating Furnaces and Soaking Pits	144	Corrosion	155
Forging, Stamping, Drawing, and Pressing	145	Economics and Statistics	156
Rolling-Mill Practice	146	Miscellaneous	157
Machinery for Iron and Steel Plant	147	Book Notices	158
		New Publications	160

ORES—MINING AND TREATMENT

Rock Drilling Bits Tungsten Carbide Tipped. (British Intelligence Objectives Sub-Committee, 1947, B.I.O.S. Final Report No. 1091: H.M. Stationery Office). Descriptions are given of German rock-drilling bits with tungsten-carbide tips and of the methods of brazing the tips to the carbon steel shanks.—R. A. R.

Diamond Drilling with Reversed Water Flushing and Sintered-Powder Drill Crowns. I. Janelid. (Jernkontorets Annaler, 1947, vol. 131, No. 5, pp. 192–195). [In Swedish]. Data are presented on comparative tests of the performance of diamond and sintered-powder drilling bits. The results demonstrate the very great advantages gained by reversing the flow of water so that it passes into the hole, along the outside of the drill and returns through the core—R. A. R.

Iron Ore Fusion Piercing. (Canadian Metals and Metallurgical Industries, 1947, vol. 10, Mar., p. 27). A short account is given of the experimental use of an oxygen blowpipe in piercing blast-holes in iron-ore deposits in the Mesabi Range. Holes 6in. in dia. and 30 ft. deep were made at the rate of 10ft./hr. in rock drilled by normal methods at the rate of 1ft./hr.—C. O.

Scraper-Loading Underground and a Comparison between Different Loading Methods. J. Hedlund, B. Wickbom, and I. Janelid. (Jernkontorets Annaler, 1947, vol. 131, No. 4, pp. 113–180). [In Swedish]. This paper reports the work of a Jernkontoret Committee appointed to review different methods of loading iron ore in mines. Scraper-loading equipment is described in detail with data on costs and efficiency under different conditions.—R. A. R.

The Dressing of Tungsten Minerals. F. B. Michell. (Mine and Quarry Engineering, 1947, vol. 13, June, pp. 169–177; July, pp. 204–210). The author discusses the general treatment of tungsten ores and gives examples from practice involving different types of flowsheet for wolfram and scheelite.—R. E.

Treatment of Idaho-Wyoming Vanadiferous Shales. S. F. Ravitz, I. W. Nicholson, C. J. Chindgren, L. C. Bauerle, F. P. Williams, and M. T. Martinson. (American Institute of Mining and Metallurgical Engineers, Technical Publication No. 2178: Metals Technology, 1947, vol. 14, June). The results of a laboratory investigation of the possibility of extracting vanadium from the shales of Southern Wyoming and Idaho, which contain about 1% of vanadium oxide, are presented.—C. O.

Remote Control a Feature of Ore-Conditioning Plant. A. A. Nilsen and R. Yingling. (Engineering and Mining Journal, 1947, vol. 148, Apr., pp. 74–79). Illustrated particulars are given of the ore-conditioning plant of the Tennessee Coal, Iron and Railroad Co., which handles raw ore from all the Company's mines. The plant is designed to crush, screen and blend the ores, and sinter the fines resulting from the ore-conditioning, together with accumulated flue dust. Three sintering machines are in operation. The filling and withdrawal of ore from the storage silos for blending are under remote control. —R. E.

Pilot Plant Production of High-Grade Magnetite Concentrates, Cranberry, N.C. F. D. Lamb and D. A. Woodard. (United States Bureau of Mines, 1946, Report of Investigations No. 3980), A pilot plant for the production of high grade magnetite concentrates for making sponge iron was constructed at Salisbury, North Carolina, in 1944. A description of this plant and the results achieved with it are given.—R. A. R.

Preparation as the Basis of the Smelting of Lean Acid Ores. C. P. Debuch. (Stahl und Eisen, 1947, vol. 66-67, June 19, pp. 205–212). After reviewing the mining activity, both open-cast and underground, of the lean iron ores of the Salzgitter district, the plant and processes by which the Reichswerke prepare the ore for the blast-furnace are described in detail, with data on the results obtained.—R. A. R.

TEMPERATURE MEASUREMENT AND CONTROL

Thermo-Electric Temperature Measurement. G. H. Talbot. (Society of Instrument Technology : Instrument Practice, 1947, vol. 1, May, pp. 265–270). The theory of thermo-electric temperature measurement is explained with notes on the behaviour of thermo-electric circuits. Its application to several different industrial processes is described with recommendations on the construction of sheaths for thermocouples.—R. A. R.

Determination of Molten Metal Temperatures. G. Vennerholm and L. C. Tate. (American Foundrymen's Association, 1947, Preprint No. 47-5). Several methods of measuring the temperature of molten metal are briefly reviewed and their merits and disadvantages are discussed. Detailed descriptions are then given of the equipment for two methods which are satisfactory for iron foundries. One is a modification of the Collins-Oseland radiator pyrometer and the second is a modification of the Schofield-Grace quick-immersion thermocouple.—R. A. R.

The Selection and Installation of Thermocouple Extension Lead Wires. C. C. Roberts and C. A. Vogelsang. (Industrial Heating, 1947, vol. 14, Mar., pp. 370–382 ; Iron Age, 1947, vol. 159, Mar. 6, pp. 82–84). The importance of careful selection, installation, and maintenance of the lead wires of thermocouple extensions is emphasized. The types of leads and insulation used for the common thermocouple combinations are summarized.—C. O.

Measurement of Temperature in the Open-Hearth. J. R. Green. (Blast Furnace and Steel Plant, 1947, vol. 35, Mar., pp. 344–347 ; Apr., pp. 443–447). A modified form of the platinum thermocouple for measuring the temperature of the bath of an open-hearth furnace is described and illustrated, and the value of the instrument is compared with that of an instrument using an evacuated sighting tube and a photo-electric cell. Methods of determining the temperature of furnace roofs are discussed. In the second part of the paper the determination of the character, or " radiating power " of the flame, and the measurement of checker-chamber temperature, are considered.—J. R.

The Design of Barrier-Layer, Photocell Pyrometers. H. Lund. (Journal of Scientific Instruments, 1947, vol. 24, Apr., pp. 95–97). The use of barrier-layer photocells in pyrometers for measuring high temperatures is discussed, and nomograms are reproduced which simplify the calculations necessary in the design of such pyrometers.—C. O.

A Photo-Electric Pyrometer for a Small High-Frequency Induction Furnace. T. Land and H. Lund. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 75–77). A simple pyrometer is described which uses a barrier-layer-type photo-electric cell ; it has been found to be valuable in measuring the temperature of molten permanent-magnet alloys in a small high-frequency induction furnace. The conditions from the pyrometric point of view are reasonable in this particular instance, and the pyrometer will measure the true temperature of the charge within $\pm 10°$ C. ($\pm$ 18° F.) for certain alloys.

A Temperature Controlling Device for Small Electric Furnaces. G. H. B. Lovell. (British Refractories Research Association : Transactions of the British Ceramic Society, 1946, vol. 45, Oct., pp. 333–334). A simple mechanical device for controlling the temperature of small electric furnaces by varying the ratio of the lengths of time for which the current is and is not flowing is described and illustrated.—C. O.

Measuring Surface Temperatures. G. H. Williams. (British Engineering Export Journal, 1947, vol. 29, Mar., pp. 1136–1139). Particulars are given of " Therm-index " temperature-indicating paints, and some of their practical applications are described.—R. E.

BLAST-FURNACE PRACTICE AND THE PRODUCTION OF PIG IRON

Latest British Steelworks Plant. (Iron and Coal Trades Review, 1947, vol. 154, June 20, pp. 1165–1170). Brief descriptions are given of recently built (or rebuilt) blast-furnace installations at Margam, Scunthorpe, Tees-side, Consett, and Shelton.—C. O.

Demolition and Rebuilding of No. 2 Blast Furnace at the Newcastle Steel Works. H. Hughes. (B.H.P. Review, 1947, vol. 24, Mar., pp. 8–10). The equipment and procedure used in dismantling and rebuilding a blast-furnace at the Newcastle Works of Broken Hill Proprietary Co., Ltd., in Australia, are described and illustrated. —R. A. R.

Electric Smelting Plant at Choindez. (Journal of the Iron and Steel Institute, 1947, vol. 156, June, pp. 293–298). A description is given of the Tysland-Hole electric pig iron furnace erected at Choindez in 1943 ; the information is based on the paper by E. Gehrig (*see* Journ. I. and S.I., 1947, vol. 156, May, p. 131).—R. A. R.

Electric Smelting. R. Durrer. (Journal of the Iron and Steel Institute, 1947, vol. 156, June, pp. 257–260). The development of electric smelting furnaces is outlined, with special reference to the low-shaft furnace. The reactions taking place in electric smelting are discussed and compared with those of the blast-furnace, particular consideration being given to the energy consumption in the two types of furnace. In conclusion the possibilities of the future development of electric smelting are considered.

The Development of the Blast-Furnace Profile. E. Cotel. (Iron and Steel Institute, 1947, Translation Series, No. 310). This is an English translation of a paper which was published in Royal Hungarian Palatine-Joseph University, Publications of the Department of Mining and Metallurgy, 1941, vol. 13, pp. 3–10. (*See* Journ. I. and S.I., 1947, vol. 155, Feb., p. 302).—D. R. S.

Gas Turbine Applications in Iron and Steel Works. Part II. Blast-Furnace Blowing Applications. Section I. The Axial Blower. W. H. Gibson. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 104–108). The possibilities of the straight axial-flow compressor as a blast-furnace blower are considered.—R. A. R.

Gas Turbine Applications in Iron and Steel Works. Part II. Blast-Furnace Blowing Applications. Section II. The Bled-Axial Gas-Turbine Blower. J. W. Railly. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 108–113). A single-shaft gas turbine coupled to a single bled-axial compressor meets both blast-furnace and combustion-air requirements. The characteristics and advantages of this type of blowing plant are dealt with.—R. A. R.

Gas Turbine Applications in Iron and Steel Works. Part II. Blast-Furnace Blowing Applications. Section III. Operation and Control of the Bled-Axial Gas-Turbine Blower. R. G. Voysey. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 113–118). The layout of a 50,000-cu. ft./min. gas turbo-blower of the bled-axial type, and its operation and control, are described.—R. A. R.

Blowing Oxygen-Enriched Air into the Blast Furnace. K. Neustaetter. (Blast Furnace and Coke Association of the Chicago District : Blast Furnace and Steel Plant, 1947, vol. 35, Mar., pp. 329–332). **Oxygen Enriched Blast.** K. Neustaetter. (Steel, 1947, vol. 120, May 19,

pp. 110–112, 133). A review is made of Russian and German literature dealing with oxygen-enriched blast in the blast-furnace, and the results to be expected from such practice in the United States are discussed.—J. R.

Oxygen in Soviet Blast-Furnace Practice. P. F. Dyer. (Mining Journal, 1947, vol. 229, July 12, p. 426). Summaries are given of two Russian papers, one by V. V. Kondakov (Bull. acad. sci. U.R.S.S. Classe sci. tech., 1946, pp. 1401–1420) describing experiments at the Stalin steelworks at Kuznetsk on the use of oxygen in Bessemer practice, and the other by I. P. Bardin describing the use of oxygen-enriched air in the blast-furnace. According to Kondakov, a drawback to the use of oxygen in the converter is the heavy wear, amounting to complete destruction, upon the tuyeres. Out of eighteen different types tested, made of magnesite, chrome-magnesite, and other materials, none outlasted one blow.—R. E.

Performance of a Blast-Furnace with Oxygen-Enriched Blast. M. A. Shapovalov. (Iron and Steel Institute, 1947, Translation Series No. 312). This is an English translation of a paper which was published in Kislorod, 1944, No. 1, pp. 17–31. The effect of the use of oxygen-enriched blast on the flame temperature, heat distribution, and chemical reactions in the blast-furnace is discussed in some detail, special consideration being given to the possibility of increasing the calorific value of the blast-furnace gas. Results obtained in practice in the production of converter iron and ferro-alloys with the simultaneous formation of slags suitable for the manufacture of Portland cement are reported. The use of oxygen-enriched blast in the Bessemer converter is briefly discussed.—D. R. S.

Experimental Laboratory Study on Effect of Pressure on Carbon Deposition and Rate of Reduction of Iron Oxides in the Blast-Furnace Process. L. F. Marek, A. Bogrow, and G. W. King. (American Institute of Mining and Metallurgical Engineers, Technical Publication No. 2184: Metals Technology, 1947, vol. 14, June). An account is given of a laboratory investigation of the effect of pressure on the reduction of iron oxides by gases under conditions approximating to those in the blast-furnace. Among the conclusions reached were: (1) The application of pressures above 5 atm. greatly increases the extent of carbon deposition, but there is no measurable increase at pressures below 10 lb./sq. in.; (2) the reduction of Fe_2O_3 proceeds by two steps, $Fe_2O_3 \rightarrow FeO$, and $FeO \rightarrow Fe$, the rate-determining factor being the thermodynamically available CO in contact with the oxides, so that the rate of reduction is proportional to either the linear gas velocity at constant pressure, or the pressure at constant linear velocity.—C. O.

Pressure for Pig. (Industrial Bulletin of Arthur D. Little, Inc., 1947. May, pp. 1–2). Full-scale tests carried out by the Republic Steel Corporation have shown that by operating a blast-furnace at a top pressure of 10 lb./sq. in. there was a 20% increase in output and a 12% reduction of coke consumption per ton of pig. This method of operation should prove particularly useful in offsetting the disadvantages of inferior quality coke.—M. A. V.

Blast Furnace Practice Under High Pressure Operation. J. H. Slater. (American Iron and Steel Institute: Steel, 1947, vol. 120, June 9, pp. 102–106). An account is given of the experiences of the Republic Steel Corporation with the use of high top-pressures on blast-furnaces at Youngstown and Cleveland. Reductions in coke consumption of from 130 to 255 lb. per ton of iron have been obtained when working with top pressures of 10–11 lb./sq. in. In spite of the large increase in the volume of air blown, less flue dust has been produced. The operation is still in the stages of development, but it is already obvious that the use of increased static pressures gives a flexibility of control hitherto unobtainable, and the efficiency of the process is reflected in a reduction in manufacturing cost of over $1·00 per ton of iron.—C. O.

Observations on Coke Oven and Blast Furnace Practice at the Geneva Plant. C. L. Waggoner. (Blast Furnace and Coke Association of the Chicago District: Blast Furnace and Steel Plant, 1947, vol. 35, Mar., pp. 325–328). **Coke and Iron Practice in Utah.** C. L. Waggoner. (Steel, 1947, vol. 120, Apr. 21, pp. 112–114). The measures taken at the plant of the Geneva Steel Company, Utah, to improve the quality of coke produced from the local coals, which are high in volatiles and mainly of non-coking variety, are described. The coking temperature was increased to nearly 2200° F., the coarseness of coal charged to the ovens was increased, and about 4% of pitch was added. Other problems connected with the ore and limestone used are mentioned. —C. O.

Rate of Reduction of Geneva Iron Ore. J. R. Lewis. (American Institute of Mining and Metallurgical Engineers, Technical Publication No. 2177: Metals Technology, 1947, vol. 14, June). An apparatus for determining the rate of reduction of iron ore by hydrogen is illustrated. Data obtained on the reduction of iron ores from Geneva, Utah, are shown in graphical form. The time of reduction of ore cubes, allowing for an induction period, was directly proportional to the volume. The merits of Joseph's "reduction index" and Philbrook's "slope index" methods of expressing reducibility are compared. —C. O.

PRODUCTION OF STEEL

Review of the Swiss Metal and Engineering Industries. W. M. von Orelli. (Journal of the Iron and Steel Institute, 1947, vol. 156, June, pp. 145–154). Switzerland does not possess an iron and steel industry proper, at least not an industry of a magnitude comparable with that of the traditional iron and steel countries. The works existing in Switzerland usually form part of, or are subordinated to, engineering factories. In this paper some characteristics of the industry are described; various raw-material problems, the historical development (including war-time activities) and more general problems are briefly considered in this connection.

Steel-Melting Practice. C. G. Herty. (Metal Progress, 1947, vol. 51, May, pp. 747–751). A broad outline is given of the changes in steel-melting practice in the United States in the last quarter of a century.—C. O.

The Development of Basic Converter Practice. W. Bading. (Stahl und Eisen, 1947, vol. 66–67, Apr. 24, pp. 137–149; May 22, pp. 180–186; June 19, pp. 212–223). Detailed descriptions are given of processes in which the Bessemer converter is used for refining various kinds of pig iron, not merely to produce steel, but also as part of a series of processes for the recovery of alloying elements such as vanadium, manganese, and phosphorus.— R. A. R.

The Production of Iron and Steel with Oxygen-Enriched Blast. R. Durrer. (Journal of the Iron and Steel Institute, 1947, vol. 156, June, pp. 253–256). The possibility of using oxygen-enriched blast for iron-ore smelting and for steelmaking by the Bessemer process is considered, particular reference being made to the economic conditions.

Modifications of blast-furnace and converter design and of current practice which would be necessitated by the application of oxygen-enriched blast are briefly outlined. The development of a combined smelting-converting process using oxygen-enriched blast is envisaged.

Furnaces for Steel Making. J. H. Chesters and M. W. Thring. (Journal of the Institute of Fuel, 1947, vol. 20, Feb., pp. 77–90). The development of the regenerative open-hearth furnace and the thermodynamics of steel-making are briefly surveyed. Descriptions are given of the methods used for making measurements of heat input, heat flow, and flame radiation during experimental trials with three open-hearth furnaces in commercial operation. The data obtained indicated (1) that the output of a furnace can be increased by the use of adequate instrumentation and control, (2) that there are considerable periods when the roof could withstand a greater heat input to the furnace, and (3) that the best design of gas port is that which causes the necessary amount of gas to be burnt with the shortest flame.—C. O.

Oil-Firing of Open-Hearth and Other Metallurgical Furnaces. (Iron and Coal Trades Review, 1947, vol. 154, June 20, pp. 1143–1148). Notes are given on experience and experiments with oil-fired open-hearth and heat-treatment furnaces at British steelworks.—C. O.

Report on Hot Metal Quality. (American Iron and Steel Institute, Feb., 1947 ; Contributions to the Metallurgy of Steel, No. 14). A questionnaire on the quality of hot metal for open-hearth steel furnaces was recently distributed to various steel producers in the United States ; the replies are analysed in this pamphlet.—R. A. R.

Fluidity of Furnace-Killed *vs.* Ladle-Killed Basic Open Hearth Heats. D. J. Murphy. (American Institute of Mining and Metallurgical Engineers : Blast Furnace and Steel Plant, 1947, vol. 35, May, pp. 558–559). The effects of various de-oxidizing additions and practices on the fluidity of melts at the open-hearth plant of the Scullin Steel Co. are discussed.—J. R.

Use of Oxygen in Open-Hearth Furnaces. J. J. Golden. (American Institute of Mining and Metallurgical Engineers : Blast Furnace and Steel Plant, 1947, vol. 35, May, pp. 572, 582). A study is made of some features of burner equipment for the use of oxygen at three open-hearth plants, and the corresponding operating data are tabulated.—J. R.

What About the Use of Oxygen in Steelmaking? J. D. Knox. (Steel, 1947, vol. 120, June 23, pp. 106–108, 144–146 ; June 30, pp. 86–92). The results of experiments at 24 open-hearth plants with the use of oxygen for the melting and refining of steel are summarized. Four methods have been used to shorten the melt-down period : (1) Addition of oxygen to the fuel or air in the main burners ; (2) scrap melting by directing a jet of oxygen on to preheated scrap ; (3) the use of auxiliary oxygen/fuel burners in the walls and roof ; and (4) the enrichment of air by injecting oxygen into the checkers. The speed of refining has been increased by the direct injection of oxygen either below the slag through steel pipes or on to the molten metal surface exposed by an oxygen jet. It is considered that insufficient experience is available for definite conclusions to be drawn, and that much remains to be learned concerning the economics and techniques of the oxygenation processes.—C. O.

Instrumentation. (Iron and Steel, 1947, vol. 20, June, p. 310). The preliminary recommendations of the Open-Hearth Instruments Committee of the British Iron and Steel Research Association regarding the instrumentation of open-hearth furnaces are given.—J. R.

FOUNDRY PRACTICE

Advances in the Foundry Industry. C. K. Donoho. (Metal Progress, 1947, vol. 51, May, pp. 765–770). Wartime advances and present trends in iron and steel foundry practice are reviewed.—C. O.

The Cupola as a Precision Instrument. R. C. Tucker. (Foundry Trade Journal, 1947, vol. 82, May 22, pp. 75–78). Cupola design, raw materials, and operation are discussed in relationship to promoting uniformity in melting rate, metal temperature, and metal quality from tap to tap and day to day.—J. R.

Cupola Refractories. R. A. Witschey. (Chicago Regional Foundry Conference : Foundry, 1947, vol. 75, Apr., pp. 120–123, 242–248 ; May, pp. 94–95, 232–240). Cupola-refractory practice is discussed in detail, with particular reference to choice of material, construction, cost, and maintenance.—C. O.

Saving Foundry Coke. J. A. Bowers. (Foundry, 1947, vol. 75, June, pp. 72–73, 258–260). Methods of economizing in the consumption of coke in the cupola are outlined.—C. O.

Foundry Coke and Its Relation to Cupola Melting. G. C. Creusere. (American Foundrymen's Association : Canadian Metals and Metallurgical Industries, 1947, vol. 10, May, pp. 23–25, 41–42). The author gives a short account of the manufacture and testing of coke for foundry use and an outline of the principal points of good cupola practice.—C. O.

Survey of Foundry Coke Characteristics. D. E. Krause and H. W. Lownie, jun. (American Foundrymen's Association, 1947, Preprint No. 47-33). The literature on the properties of foundry coke and the manner in which they affect cupola operation is reviewed. The classified bibliography contains 158 references.—R. A. R.

Thermochemical Analysis of Combustion in a Cupola. H. E. Flanders. (American Foundrymen's Association, 1947, Preprint No. 47-50). A study of the thermochemical reactions in a cupola, based on certain simplifying assumptions, has resulted in the formulation of two equations by which the heat of combustion and maximum combustion temperature may be calculated. These equations were developed after a large number of calculations of these quantities were made, and they conform to the calculated data within a reasonable degree of accuracy. It is possible, with such equations, to estimate the relative effect of preheating of blast air, or of removal of moisture from the blast, on the heat of reaction or the maximum temperature attainable.—R. A. R.

Cupola Melting Phenomena. E. V. Somers and D. W. Gunther. (American Foundrymen's Association, 1947, Preprint No. 47-51). An investigation of the manner in which the tapping temperature affects the properties of cupola-melted cast irons is reported.—R. A. R.

Cupola Operation. W. Chrétien-Howard. (Iron and Steel, 1947, vol. 20, June, p. 306). Specimen calculations are given for the estimation of the final content of carbon, silicon, phosphorus, sulphur, and manganese in cupola metal.—J. R.

Mechanized Melting Methods for Foundries. A. W. Gregg. (Iron Age, 1947, vol. 159, May 8, pp. 60–65). Mechanized methods, with special reference to cupola charging, for increasing the production of foundries are described.—R. A. R.

The Annealing of Whiteheart Malleable Iron by a Gaseous Mixture. G. Joly. (Fonderie, 1946, Oct., pp. 335–372). French practice has been to use batch-type coke or pulverized-coal furnaces which are inefficient as regards fuel, labour, and throughput. The author

recommends the use of controlled atmospheres in electric or gas furnaces as described by A. G. Robiette and by I. Jenkins and S. V. Williams (*see* Journ. I. and S.I., 1945, No. II, pp. 81A and 80A). These two papers are summarized.—R. A. H.

Graphitization of White Cast Iron—Effect of Section Size and Annealing Temperature. R. Schneidewind, D. J. Reese and A. Tang. (American Foundrymen's Association, 1947, Preprint No. 47-7). A study of the time necessary for first-stage annealing of sections of white-iron step bars indicated the great influence of the cooling rate of the casting during solidification. The cooling rate was assumed to be proportional to the ratio of volume to surface area of the casting. It was found that the logarithm of this ratio is directly proportional to the logarithm of the annealing time and to the grain size of the metal.—R. A. R.

Malleable Foundry Finishing and Inspection. T. E. Poulson. (American Foundrymen's Association, 1947, Preprint No. 47-8). The author reviews steps and procedures for delivering a satisfactory product, beginning with the raw casting after pouring and following through the cleaning, finishing, and inspection operations.

Material Handling in a Malleable Foundry Processing Department. N. J. Henke. (American Foundrymen's Association, 1947, Preprint No. 47-22). Good methods of handling material in a malleable foundry processing department are outlined. The paper covers methods of moving castings within the department, storage of rough and finished castings, operator fatigue, and working conditions. The methods of material handling discussed include belt conveyors, trucking, magnetic cranes, and manual movement.—R. A. R.

Mechanized Malleable Foundry Finishing and Inspection. D. F. Sawtelle. (American Foundrymen's Association, 1947, Preprint No. 47-24). This paper describes finishing operations in a mechanized malleable iron foundry, from mould shake-out to finished casting. Following the shake-out, in which some sprues are broken from the castings, are two tumbling barrels in tandem. These barrels automatically load and discharge, and cool and clean the castings before transferring them to a conveyor belt. The belt passes the cupolas where the sprue is removed by hand into a storage bin.—R. A. R.

Modernizes Malleable Iron Foundry. E. F. Ross. (Foundry, 1947, vol. 75, May, pp. 72–77, 222–226). The reorganization of a malleable iron foundry at South Bend, Indiana, is described. By the maximum utilization of mechanical handling for raw materials, moulds, and metal, and strict adherence to the principle of straight-line flow, the output of finished castings, 4–12 lb. in weight, has been almost doubled.—C. O.

Specifications for Malleable Iron Castings. J. E. Linabury. (Foundry, 1947, vol. 75, June, pp. 84, 224–230). The factors to consider in making specifications for malleable iron castings are discussed.—C. O.

Acid Electric Steel for Castings. S. F. Carter and C. R. Donoho. (Electrochemical Society, 1947, Preprint No. 91-12). The production of steel for castings in the acid electric arc furnace was studied, with special reference to the effect of variations in melting and deoxidation practice on the ductility of the steel. The experimental data, which were evaluated statistically, prove that such variations have a definite effect, the highest ductility being obtained by maintaining a relatively oxidizing bath throughout the heat and deoxidizing by the simultaneous addition of silicon and manganese shortly before tapping. Deoxidation with aluminium, though valuable in preventing pinholes, tends to lower the ductility.—D. R. S.

Producing Steel Castings without Venting Molds. J. R. Adams. (American Foundryman, 1947, vol. 11, Mar., pp. 26–29). **Should Moulds be Vented ?** J. R. Adams. (Foundry, 1947, vol. 75, June, pp. 86–87, 238–244). The practice of venting the moulds for steel castings is considered by the author to be of no advantage, or even detrimental, because of the voids and dirt frequently associated with vents. The solution to the problem of pin-hole porosity lies in making the right kind of steel in the furnace.—C. O.

Influence of Selenium on Sulphide Inclusions and Ductility of Cast Steel. A. P. Gagnebin. (American Foundrymen's Association, 1947, Preprint No. 47-41). In addition to refining the grain, selenium has the specific ability to coalesce the intergranular sulphides in low-oxide, well-killed cast steel, and thereby to improve its ductility. A demonstration of this phenomenon under a variety of conditions, and a theory for the mechanism of its occurrence, are discussed. A deoxidation practice for cast steel utilizing selenium is proposed and consists of ladle additions of 0·08% of calcium and 0·05% of selenium.—R. A. R.

Segregation in Small Steel Castings. H. F. Bishop and K. E. Fritz. (American Foundrymen's Association, 1947, Preprint No. 47-46). Pronounced segregation of carbon, and to a minor degree of the other alloying elements, has been found in small steel castings adjacent to the riser contacts. This segregation is indicative of potential shrinkage and occurs when restricted riser contacts are used. Examples of segregated zones are shown in which the carbon content is nearly twice as great as the carbon content of the base metal. It is concluded that in castings where such segregation occurs, the riser contact consists of solid low-carbon dendrites surrounded by liquid high-carbon metal during the latter stages of solidification. To supply the last feed metal required by the casting, the interdendritic material is drawn into the casting to form the segregated zone.—R. A. R.

The Control of Moulding and Core Sands. R. Guérin. (Fonderie, 1946, Feb., pp. 43–52). The author considers that frequent tests for moisture, green strength, and fineness are adequate for mouldings sands. The A.F.A. (American Foundrymen's Association) fineness index is preferred. Permeability tests are not regarded as a sure guide to quality control. As regards core sands, tests for fineness and dry bending strength are recommended. Two French silica sands bonded with 1% of linseed oil diluted with a tar fraction were tested for dry bending strength, a firing temperature of 240° C. being used. J. J. Sheehan's work on the effect of grain-size of core sands on the quality of castings (*see* Journ. I. and S.I., 1939, No. II., p. 73A) is quoted. As a practical point, the author suggests that sand testing should be carried out by staff outside the foundry.—R. A. H.

Sand Control. G. A. Zabel. (Foundry, 1947, vol. 75, Apr., pp. 110–113, 150–152). A short account is given of the sand-control methods used by the Universal Foundry Co., Oshkosh, Wisconsin. Most of the moulding sand used is synthetic, and its composition is very carefully controlled by frequent testing, using the standard methods of the American Foundrymen's Association.—C. O.

Adopts Chemically Coated Molding Sand. W. G. Gude. (Foundry, 1947, vol. 75, May, pp. 66–71). A general account is given of the experience of a Lynchburg, Virginia, grey-iron foundry with the use of moulding sands coated with a non-thermo-setting resin, known as Westonite.—C. O.

Preparation of Foundry Sands for Market. F. P. Goettman. (American Foundrymen's Association, 1947, Preprint No. 47-12). Foundry sands are classified as bonded, semi-bonded, and washed. Only sands possessing the proper qualities, such as grain size, refractoriness, bond, permeability, and durability, may be used; thus every deposit is not suitable. A given deposit should be thoroughly mapped and prospected before development in order to provide a basis for planning a flexible mining and processing method. The importance of processing, field supervision, and laboratory control is stressed. Typical deposits of each of the three classifications of foundry sands and practices used in their preparation are discussed. Emphasis is placed on thorough removal of overburden, control of feed, and adjustment of classifiers.

Physical Properties of Molding Sands. G. R. Gardner. (American Foundrymen's Association, 1947, Preprint No. 47-13). Whilst standard tests on foundry sand mixtures assist in maintaining the quality of finished moulds, the moulder's technique is equally important. In this paper data are presented on the manner in which various combinations of moisture content and ramming energy affect the properties of the finished mould.—R. A. R.

A Study of the Precision of Sand Test Data. R. E. Morey and C. G. Ackerlind. (American Foundrymen's Association, 1947, Preprint No. 47-18). Reproducibility of precision of tests used in the evaluation of foundry moulding sands was examined by statistical methods. Shewhart's method for relatively small samples was found particularly suitable for this purpose.—R. A. R.

New Tentative Standards for Grading and Fineness of Sands. R. E. Morey. (American Foundrymen's Association, 1947, Preprint No. 47-29).

Density of Molding Sands. H. W. Dietert, H. H. Fairfield, and E. J. Hasty. (American Foundrymen's Association, 1947, Preprint No. 47-39). An investigation on the effect of ramming density on the properties of moulds and castings is reported. The paper covers in particular: (*a*) The effect of the sand density on the solidification of the metal; (*b*) the effect of moisture, clay, grain-size, and other variables on the density of the sand; and (*c*) the effect of the ramming method on the sand density.—R. A. R.

Thermal Conductivities of Three Sands. C. F. Lucks, O. L. Linebrink, and K. L. Johnson. (American Foundrymen's Association, 1947, Preprint No. 47-55, pp. 10–13). The thermal conductivities of three moulding sands were determined over the 750–2250° F. range and were found to increase with temperature.—R. A. R.

Gypsum Cement—Practical Patternmaking Applications. E. H. Schleede. (American Foundryman, 1947, vol. 11, Jan., pp. 46–50). Practical applications of gypsum cement in pattern-making are illustrated. Among the examples quoted are the checking of a core-box by the "booking" method, the correction of a pattern for metal shrinkage, and the production of material for patterns.—C. O.

Some Jobbing Foundry Methods. (Iron and Steel, 1947, vol. 20, Apr., p. 128). The use of the old casting as a pattern in foundry practice is discussed.—J. R.

What Kind of Patterns? J. E. Johnson, L. G. Gustafson, and V. E. Zang. (Steel Founders' Society of America: Foundry, 1947, vol. 75, Mar., pp. 70–71; Apr., pp. 128–131; May, pp. 86–89). In the first two parts of the article patterns for jobbing quantities of small, medium, and large castings are illustrated. The concluding section deals with patterns for long production runs. —C. O.

Pattern Purchase Considerations. W. G. Schuller. (American Foundrymen's Association, 1947, Preprint No. 47-38).—R. A. R.

HEATING FURNACES AND SOAKING PITS

Development of an Oil-Fired Pile-Heating Furnace for the Wrought Iron Industry. L. G. A. Leonard. (Metallurgia, 1947, vol. 36, June, pp. 95–96). A short description is given of a furnace designed for the heating of piles of bars in the manufacture of wrought iron. The batch-type furnace has two chambers which are used alternately for preheating and heating, the waste gases from one chamber passing under the arch which divides them into the second chamber. The thermal efficiency of the furnace, which is heated by two low-pressure oil-burners, is about three times that of the coal-fired furnace it replaced.—C. O.

Melting and Reheating Furnaces. (Iron and Coal Trades Review, 1947, vol. 154, June 20, pp. 1171–1180, 1192). Present trends in the construction of furnaces for hot-working, annealing, stress-relieving, and hardening steels are illustrated by examples of recent installations at British steelworks.—C. O.

Gas-Heating Speeds Seamless Tube Production. E. S. Kopecki. (Iron Age, 1947, vol. 159, June 5, pp. 64–68). A detailed description is given of a continuous gas-fired tube-heating unit developed by the Babcox and Wilcox Tube Co. for reheating seamless steel tubing during the final stages of drawing. Twelve of these "Selas" units in series handle the entire output of a piercing mill, heating 4-in. dia. tubes from 1200–1400° F. to 2000–2200° F. at rates of 50–150 ft./min.—C. O.

Heating and Melting Furnace Controls. C. G. Bigelow, jun. (Iron and Steel Engineer, 1947, vol. 24, June, pp. 44–49). The costs of installing control instruments on heating and melting furnaces and the value of the savings effected by the use of adequate instrumentation are compared.—C. O.

Heat Problems in the Steel Industry. V. Paschkis. (Iron and Steel Engineer, 1947, vol. 24, May, pp. 83–87). The author mentions several complex heat-flow problems encountered in the steel industry, such as in the melting furnace, the soaking pit, and ingot solidification. Methods of solving these problems are discussed—in particular the "heat and mass flow analyser," which is based upon an analogy between the flow of heat and of electric current.—C. O.

Two Opinions on Conditions for the Successful High Frequency Hardening of Steels. (Sheet Metal Industries, 1947, vol. 24, June, pp. 1196–1199). In a paper on the high-frequency induction heat-treatment of steel, R. J. Brown stated that a plain carbon steel with 0·40–0·45% of carbon was quite satisfactory and that in order to achieve the best results in the application of the method it is necessary to heat the surface layers at the maximum possible speed (*see* Journ. I. and S.I., 1947, vol. 156, Aug., p. 577). Exception is taken by J. D. Jevons to these two statements and correspondence on the subject is published under the above title.—R. A. R.

A New Principle in Induction Heating. T. E. Lloyd. (Iron Age, 1947, vol. 159, June 12, pp. 46–48). A general description is given of the use of the Gordon Trigatron, an induction-heating apparatus which employs a specially designed gaseous three-element tube for the conversion of 60-cycle into radio-frequency energy. Among the advantages claimed for the instrument are constant work-coil power output, stepless power control, and inexpensive work-coil construction.—C. O.

Some Applications of Induction Heating. G. W. Seulen. (Iron and Coal Trades Review, 1947, vol. 154, May 30, pp. 979–984; June 6, pp. 1029–1032). A review, based on work carried out in Germany during and since the war, is made of the scope for induction heating in the processing of steel. The techniques discussed can be applied to surface hardening, hot-working and heat-treatment, the partial heating of work for forging or heading, the softening of oxy-acetylene cut edges, and the brazing of tungsten carbide tips to tool shanks.—R. A. R.

Mercury Arc Frequency Changing Equipment for Induction Heating. S. R. Durand. (Iron and Steel Engineer, 1947, vol. 24, Apr., pp. 102–108). Mercury-arc frequency-changing equipment for induction heating is discussed. Although many advantages are, in general, to be obtained, a careful analysis of the problem involved should always be made before such equipment is used in untried applications.—J. R.

Infra-Red Drying. J. D. Keller. (Industrial Heating, 1947, vol. 14, Apr., pp. 568–584). Illustrated descriptions are given of the common industrial sources of infra-red radiation, including electric bulbs, radiant gas-burners, radiant gas-fired tubes, and gas-fired trough-type burners. The efficiencies of the various types are compared.—C. O.

FORGING, STAMPING, DRAWING, AND PRESSING

The Strengthening of Steel by Correct Hot-Working Conditions. K. F. Grachev. (Vestnik Machinostroenia: Engineers' Digest, American Edition, 1947, vol. 4, June, p. 286). Experiments were conducted to determine whether an ideal grain size and optimum mechanical properties can be obtained in forged steel without employing severe deformation at high temperature plus slight deformation at close to the critical temperature. The whole of the deformation should be carried out at high temperature if the aim is to obtain a uniform fine-grained structure.—R. A. R.

Wartime Forging. E. S. Gregory, W. J. Davies, and E. R. Wellburn. (Iron and Steel, 1947, vol. 20, May, pp. 189–190, 192). As an example of improvisation of plant, a description is given of the war-time manufacture of bomb suspension lugs by forging in a hydraulic nosing press originally intended for the production of small bombs and shells.—J. R.

Steel Forging. J. M. Mowat. (Iron and Steel, 1947, vol. 20, Apr. pp. 157–160). Some notes on recent German steel-forging practice are given.—J. R.

The Cold Heading Process. F. Rhodes. (Machinery, 1947, vol. 70, May 1, pp. 449–451). The advantages and extending field of application of cold-heading, upsetting, and subsequent operations in the production of bolts, screws, and similar small components are referred to. Stages in the manufacture of a bolt are shown. Illustrations of some miscellaneous parts made by this process, and six general rules to be observed when designing parts for cold-heading are given—R. L. B.

Cold-Heading Makes Pull Rods Faster. R. Le Grand. (Machinist, 1947, vol. 91, June 7, pp. 186–187). The details of a method of making motor-car engine tie rods by cold-heading are illustrated.—C. O.

Manufacture of Cylinders and Pressure Vessels in Germany by the Hot Spinning Method. (British Intelligence Objectives Sub-Committee, 1947, F.I.A.T., Final Report No. 527 : H.M. Stationery Office).

The Corrugating of Sheet Metal. J. E. Kiefer. (Iron and Steel Engineer, 1947, vol. 24, Apr., pp. 95–101). Problems encountered in the production of rolled and pressed corrugated sheet metal are discussed, and improvements in equipment and methods are described. —J. R.

Press Shop Operations in Bicycle Production. (Machinery, 1947, vol. 71, July 10, pp. 31–38). The sequence of pressing operations involved in the production of steel bottom brackets and rear-wheel hubs at the bicycle factory of the Raleigh Cycle Co., Nottingham, is described and illustrated.—C. O.

Plastic Coating Expedites Stainless Stamping Operations. W. A. Phair. (Iron Age, 1947, vol. 159, May 15, pp. 47–51). A description is given of the use of a removable plastic coating on stainless steel sheets. The coating remains intact during pressing operations and reduces the power required for deep-drawing. It also protects the surface.—R. A. R.

Steel Truck Body Members Standardized for Mass Production. N. E. Cole. (Steel, 1947, vol. 120, May 5, pp. 96–99, 124). The mass production of parts for motor lorries employing standardized steel stampings is described and illustrated.—R. A. R.

Forging Die Design. J. Mueller. (Steel Processing, 1947, vol. 33, Mar., pp. 164–166). A description is given of the process of "die-typing," in which a master model of hardened steel is forced by a steam hammer or a press into a heated steel block, to produce an impression which can be used as a female die.—C. O.

Forging Die Design. J. Mueller. (Steel Processing, 1947, vol. 33, May, pp. 294–296). In the die-forging of large numbers of a single part it is sometimes economical to make the die blocks with more than one impression; this creates the problem of the best distribution of the impressions over the die surface so as to distribute the stresses uniformly. Examples of this "grouping" are described.—R. A. R.

Calculating Commercial Tolerances for Impression Die Forgings. (Iron Age, 1947, vol. 159, June 12, p. 57). A chart for calculating the commercial tolerances for closed-impression die forgings of any shape and up to 500 lb. in weight is presented.—C. O.

Forging Production Readings Provided by Combustion Safeguard. (Steel, 1947, vol. 120, June 23, p. 112). An application of a photo-electric cell circuit, of the type used in combustion-control apparatus, for the counting of hot forgings on a conveyor is briefly described.—C.O.

Mechanical Cold Drawbenches for Ferrous and Non-Ferrous Tubes. G. W. Garwig and A. L. Thurman. (Blast Furnace and Steel Plant, 1947, vol. 35, Mar., pp. 348–354, 356; Apr., pp. 461–466; May, pp. 549–552). The history and development of the modern drawbench are reviewed and the component parts are described and illustrated. Auxiliary drives and electrical and control equipment are discussed.—J. R.

"Fluid Punch" Draws Sheet in One Operation. (Steel, 1947, vol. 120, Apr. 14, pp. 90–91, 118). A process is illustrated by which it is possible to draw and emboss sheet metal into shallow shapes or to draw cone-shaped or tapered stampings, in a single operation using water under very high pressure instead of a mechanical punch. —C. O.

Experimentation on Tube Drawing with a Moving Mandrel. G. Espey and G. Sachs. (Journal of Applied Mechanics, 1947, vol. 14, June, pp. A-81–A-87). Experiments are described which were designed to test a theoretical analysis of the process of tube-drawing with a moving mandrel. The relations between the drawing stress and the reduction in area and the angle of tapered dies were determined for metals giving varying degrees of strain-hardening. The data obtained

show that tube-drawing with a mandrel can be analysed in the same manner as other drawing processes.—C. O.

Obstacle in Cleaning Drawing Compounds from Enameling Iron. W. H. Pfeiffer and V. A. Williamitis. (Journal of the American Ceramic Society, 1947, vol. 30, Mar. 1, pp. 90–94). A method is described for reproducing the conditions under which die lubricants form a deposit, insoluble in water and alkaline cleaners, but soluble in organic compounds, on sheet iron of enamelling quality. Suggestions for preventing deposits are made.—R. A. R.

The Oldest and the Newest. (Wire and Wire Products, 1947, vol. 22, Mar., pp. 213–215). An illustrated description is given of a modernized New England wire mill.—C. O.

Wickwire Brothers at Cortland. K. B. Lewis. (Wire and Wire Products, 1947, vol. 22, June, pp. 427–430, 463–464). The commercial and technological history of the wire mills of Wickwire Brothers, Incorporated, at Cortland, New York, is outlined.—C. O.

Servicing of Dies. (Industrial Diamond Association's Committee of Diemakers: Wire and Wire Products, 1947, vol. 22, June, pp. 432–433, 462–463). Problems of the maintenance of diamond wire-drawing dies are discussed.—C. O.

The Present State of Knowledge of the Mechanics of Wire-Drawing. E. Siebel. (Stahl und Eisen, 1947, vol. 66–67, May 22, pp. 171–180). The literature on wire-drawing and plastic deformation is reviewed. Using the forces in shear arising in the material as it enters and leaves the die, formulæ for calculating the power requirements and stress distribution are developed. Die friction, lubrication, the temperature distribution across the wire, and drawing with back-pull are discussed. —R. A. R.

Research on the Mechanical Working of Metals. (British Intelligence Objectives Sub-Committee, 1947, Final Report No. 1177: H.M. Stationery Office). An account is given of the interrogation of Dr. Werner Lueg, Deputy Head of the Mechanical and Technological Department of the Kaiser-Wilhelm-Institut für Eisenforschung, on the subject of rolling-mill and wire-drawing research done in Germany during the war. —C. O.

ROLLING-MILL PRACTICE

The Flow of Metals Through Tools of Circular Contour. G. Sachs and L. J. Klingler. (Journal of Applied Mechanics, 1947, vol. 14, June, pp. A-88–A-98). A mathematical analysis is given of all the variations of commercial forming processes in which metals flow through an opening between the surfaces of parallel cylinders.—C. O.

Rolling Mills and Auxiliary Plant. (Iron and Coal Trades Review, 1947, vol. 154, June 20, pp. 1181–1191). Nearly every aspect of modern British rolling practice is illustrated by examples of recently installed equipment.—C. O.

Motor Selection for a Rolling Mill. L. H. Berkley. (Electrical Engineering, 1947, vol. 66, May, pp. 444–447). The considerations which led to the choice of a 200-H.P. low-slip motor for a brass-slab rolling mill are outlined.—C. O.

Discussion of Mill-Motor Standards. F. W. Cramer. (Iron and Steel Engineer, 1947, vol. 24, Mar., pp. 67–71). Following a suggestion made to the Association of Iron and Steel Engineers by the General Electric Co. that certain changes should be made to the standard Association of Iron and Steel Engineers ratings for mill motors, a questionnaire on the subject was drawn up and submitted to 31 steel-mill engineers and manufacturers. The proposed changes are outlined, and the replies to the questionnaire are analysed.—J. R.

Power Requirements for Rolling and for Mill Trains. A. Geleji. (Iron and Steel Institute, 1947, Translation Series, No. 311). This is an English translation of a paper which was published in Royal Hungarian Palatine-Joseph University, Publications of the Department of Mining and Metallurgy. 1940, vol. 12, pp. 192–212. (*See* Journ. I. and S.I., 1947, vol. 155, Feb., p. 308).—D. R. S.

Speed Stability of Motors for Continuous Mills. F. E. Crever and T. M. Linville. (Iron and Steel Engineers, 1947, vol. 24, June, pp. 50–58). The effect of sudden variations in torque on the speed of D.C. motors for continuous rolling mills is explained. To obtain the uniformity of speed which is essential to prevent looping or stretching, it is necessary to apply counter-torque, either mechanically or from the electrical characteristics of the motor itself.—C. O.

Automatic Limiting of Grid Overloading Caused by Rolling-Mill Motors. J. Perrin. (Bulletin Sécheron: Engineers' Digest, American Edition, 1947, vol. 4, June, pp. 283–286). Automatic devices are described which experience has shown to be completely reliable in limiting the overloading of the grid when mill motors are subjected to sudden loads.—R. A. R.

Calibrating a Tramrail Section by Proportion Factors. C. Holzweiler and A. Keil. (Stahl und Eisen, 1947, vol. 66-67, June 19, pp. 223–226). The method of using proportion factors for calibrating rolls is described with an example of calibrating rolls for a tramrail section 180 mm. high with a 180-mm. base.—R. A. R.

Modernization at the Sheffield Forge and Rolling Mills Co., Ltd. (British Steelmaker, 1947, vol. 13, Apr., pp. 187–191). **New Rod Mill.** (Iron and Steel, 1947, vol. 20, Apr. pp. 145–147). A description is given of a 14-in., four-stand, two-high, centralized-oil-lubricated merchant bar mill recently installed at the works of the Sheffield Forge and Rolling Mills Co., Ltd.—J. R.

Rod Mills and Rod Mill Roll Design. R. E. Beynon. (Iron and Steel Engineer, 1947, vol. 24, June, pp. 74–100). A comprehensive review is made of the development and design of rod mills, with many illustrations of mills and rolls designed from 1751 and onwards.—C. O.

Plate Mills. C. F. Buente. (United Effort, 1947, vol. 27, May, pp. 7–10). Illustrated descriptions are given of plate mills built by the United Engineering and Foundry Co., of Pittsburgh.—M. A. V.

Machinery for Roller Leveling Flat Rolled Metal. A. J. Wardle, jun. (Iron and Steel Engineer, 1947, vol. 24, June, pp. 61–65, 68). A review is made of the machines used for levelling flat-rolled material since the introduction of roller-levelling about 1860.—C. O.

Washington Steel Starts Sendzimir Mill. (Iron Age, 1947, vol. 159, Apr. 24, pp. 63–64). A very brief account is given of a 36-in. Sendzimir mill which has just been put into operation at Washington, Pennsylvania. The mill is expected to roll about 1500 tons of extra-light-gauge stainless-steel sheet per month.—C. O.

New Company Makes Stainless Steel Sheets and Strip. (Steel, 1947, vol. 120, Apr. 28, pp. 118–123). A description is given of a new rolling mill for rolling stainless steel strip and sheets up to 36 in. wide, which has been installed by the Washington Steel Corporation. The cold-rolling is done in a Sendzimir mill, of which many illustrations are given.—R. A. R.

The Heating and Rolling of Strip Steel. P. Carnahan. (Iron and Steel Engineer, 1947, vol. 24, Apr., pp. 71–75). Modern continuous-wide-strip-mill practice is outlined, and some operating principles are discussed.—J. R.

New Tandem Cold Mill Rolls Strip at Mile-a-Minute. J. C. Sullivan. (Steel, 1947, vol. 120, June 2, pp. 118–124). A new 42-in., 5-stand, 4-high tandem cold-rolling mill in operation at Weirton, West Virginia, is described. The mill is capable of rolling strip 0·0006–0·015 in. thick at speeds up to 5000 ft./min. Each work roll is driven individually by its own motor.—C. O.

The First "Mile-A-Minute" Tandem Cold Mill. C. L. Buente. (United Effort, 1947, vol. 27, June, pp. 7-10). The new five-stand four-high tandem cold mill of the Weirton Steel Company is described and illustrated. This is intended chiefly for tinplate, but it can also roll sheet gauges. It is claimed to be the fastest mill of its type in the world today. A very brief summary of the history of tinplate manufacture is included.—M.A.V.

Flat Car Conveyor System Serves 66-Inch Hot Strip Mill. (Steel, 1947, vol. 120, June 16, p. 98). A very short description is given of the conveyor system connecting the 66-in. hot strip mill and the pickling plant at the works of the Weirton Steel Co., Weirton, West Virginia. One hundred and twenty-six small flat cars are connected together on an endless oval track 636 ft. long, below the floor level—C. O.

80-In. Cold-Reduction Reversing Strip Mill at Messrs. John Summers and Sons, Ltd., Shotton, Chester. (Journal of the Iron and Steel Institute, 1947, vol. 155, Mar., pp. 442–444).

The Effect of Speed of Rolling in the Cold-Rolling Process. H. Ford. (Journal of the Iron and Steel Institute, 1947, vol. 156, July, pp. 380–398). Tests have been made on a 0·2% carbon steel, a 0·07% carbon steel, and high-conductivity copper rolled at various speeds up to 300 ft./min. in successive passes and with a constant percentage reduction in each pass. The results confirm previous researches in that, with pass reductions up to 60%, the rolling pressure required for a first pass is substantially independent of the speed of rolling. In subsequent passes, however, after work-hardening and when the strip is thin, roll pressure is markedly dependent on speed, and is highest at the lower speeds. The corresponding energy consumption also depends on speed, but to a smaller extent and in a more complex manner. The paper thus establishes on a quantitative basis the well-known facts that roll force, and to a lesser extent, roll torque, are dependent on speed when rolling fairly thin strip. Some possible causes of these effects, *viz.*, dependence of yield stress on rate of deformation, effect of temperature rise resulting from the work of deformation, roll flattening, and lubrication are discussed, and all are regarded as not providing complete explanations. It is concluded that these phenomena which undoubtedly contribute to the occurrence of gauge variations during acceleration and deceleration when rolling thin strip, require further consideration.

Reversing and Tandem Cold Mills. M. D. Stone. (Iron and Steel Engineer, 1947, vol. 24, May, pp. 65–72). After tracing the development of reversing and tandem cold-rolling mills, the author compares and contrasts their characteristics. The reversing mill is considered to be more flexible and to require less capital investment, whereas the tandem-type mill operates at a lower cost and gives greater production.—C. O.

Razor Strip. (Iron and Steel, 1947, vol. 20, Apr., pp. 143–144). The processes involved in the manufacture of razor blades from the melting of the steel to the production of the finished article, are outlined and briefly described.—J. R.

German Razor Blade Industry. (British Intelligence Objectives Sub-Committee, 1947, Final Report No. 1256 : H.M. Stationery Office). Details are given of the machines and processes employed in Germany for the manufacture of safety razor blades.—R. A. R.

MACHINERY FOR IRON AND STEEL PLANT

Water-Tube Boilers for Iron and Steel Works. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 90–97). An attempt is made to cover, in a general descriptive manner, modern water-tube boiler-plant practice with special reference to its application to the present conditions existing within the iron and steel industry.—R. A. R.

Steam Turbines for Iron and Steel Works. I. V. Robinson. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 81–89). This paper was presented on behalf of the British steam turbine builders, members of the Turbine Section of the British Electrical and Allied Manufacturers' Association. Indications of what may be the ultimate developments in power stations in iron and steel works are given and recommendations on turbine design and practice for steelworks are made.—R. A. R.

Gas Turbine Applications in Iron and Steel Works. Part I. Power Applications. A. T. Bowden. (Journal of the Iron and Steel Institute, 1947, vol. 156, May, pp. 98–103). The design and layout of gas turbines of the open-cycle type, using blast-furnace gas, coke-oven gas, heavy diesel or gas oil, and selected part-residuals of the "pool fuel" type, for steelworks power requirements are considered. (Abstracts of the papers forming Part II on blast-furnace blowers will be found on p. 140).—R. A. R.

The Future of the High Temperature Steam Piping Expansion Problem. F. A. Scanlan. (Journal of the American Society of Naval Engineers, 1947, vol. 59, Feb., pp. 48–56). Two designs of expansion joints of the corrugated or bellows type for the expansion of high-temperature pipelines where space is limited are shown. The joints are of special steel or Inconel. One of the designs shown is capable of withstanding 1000 lb. per sq. in. pressure with an operating temperature range up to 1600° F.—R. E.

Gas-Turbine Development in Switzerland. A. Meiler. (Iron and Coal Trades Review, 1947, vol. 154, June 20, pp. 1204–1207). Swiss contributions to the development of the gas turbine are reviewed. In recent years Swiss engineers have concentrated mainly upon turbines for stationary and marine applications.—C. O.

Accessory Plant and Processes. (Iron and Coal Trades Review, 1947, vol 154, June 20, pp. 1193–1199). Short accounts are given of modern developments in plant and processes closely connected with steelworks, such as heavy engineering, pickling and tinning, extraction, and internal transport.—C. O.

Gearing for Steel Mill Auxiliaries and Cranes. L. J. Collins. (Iron and Steel Engineer, 1947, vol. 24, May, pp. 73–79). Theoretical and practical considerations influencing the choice of pressure angle for gears required to transit maximum power in the minimum of space are discussed. The 20° tooth form is shown to possess several advantages over the more common 14·5° type.—C. O.

Steel Works Maintenance Shops in Germany. (British Intelligence Objectives Sub-Committee, 1947, Final Report No. 1307 : H.M. Stationery Office). Descriptions are given of the layout and machines in the maintenance shops of four important German steelworks and steel-plant manufacturers.—R. A. R.

WELDING AND FLAME-CUTTING

Modern Electrodes for Welding Iron and Steel. E, Rollason. (British Engineering Export Journal, 1947. vol. 29, May, pp. 1438–1446). The author surveys the main characteristics of British electrodes introduced to meet developments in the welding of steel. These are : (*a*) The use of unskilled labour necessitating the use of electrodes which are easy to operate ; (*b*) faster welding involving the use of deep-penetration electrodes, high currents, and larger electrodes ; (*c*) the welding of high-tensile steel ; and (*d*) the use of hard-facing alloys. The effects of hydrogen and its influence on the propagation of cracks in welds are shownd iagrammatically. Tables are included which show the composition and properties of hard-facing electrodes, classification of electrodes for the welding of cast iron, and suitable electrodes for welding various types of stainless and heat-resistant steel.—R. E.

The Development and Improvement of Spot Welding Electrodes. G. F. James. (Sheet Metal Industries, 1947, vol. 24, Jan., pp. 159–166 ; Feb., pp. 392–398 ; June, pp. 1227–1230). After a general description of the resistance-welding process and of the functions of the electrode, an account is given of an investigation into the optimum welding conditions for several types of copper-alloy electrodes and of the effect of electrode tip diameter and life upon the quality of welds.—C. O.

Development and Applications of Lime-Ferritic Electrodes. D. L. Mathias and A. P. Bunk. (Welding Journal, 1947, vol. 26, Mar., pp. 209–217). An account is given of the development, composition, and metallurgy of organic and mineral electrode coatings. The electrodes of the type known as "lime-ferritic," in which the flux is mainly limestone, together with fluorspar, a deoxidizer, and/or alloying-element stabilizers, slag modifiers, and binding solids, have solved many of the problems associated with the welding of high-sulphur, air-hardening, and complex alloy steels. The low-temperature impact properties of weld deposits from unalloyed lime-ferritic electrodes are compared with those of deposits from conventional heavy-coated electrodes.—C. O.

Spotweld Analyzer for Maintenance Work. D. F. Hays. (Iron Age, 1947, vol. 159, June 12, pp. 49–51). A portable and easily operated equipment for completely analysing the performance of spot-welding machines under service conditions is described.—C. O.

Resistance Welding of Stainless Steel—2. Seam-Welding Variables. J. J. Riley. (Machinist, 1947, vol. 90, Apr. 20, pp. 2088–2092). The heat disposal problem is mentioned. Some hints on seam-welding practice and the machines employed are given. The variables are classified, and the cycle of operations and the hazards encountered are depicted.—R. L. B.

Projection Welding of Low Carbon and Stainless Steel Sheet. M. L. Ochieano. (Product Engineering, 1947, vol. 18, Mar., pp. 124–126). Data are given on the location and dimensions of the projections required for the projection-welding of low-carbon and stainless-steel sheets 0·025–0·125 in. thick.—C. O.

The Relationship between Welding Conditions and the Strength and Quality of Single Spot Welds in Deep Drawing Quality Mild Steel Sheet in Thicknesses from 20 to 14 S.W.G. A. J. Hipperson. (Transactions of the Institute of Welding, 1947, vol. 10, Apr., Supplement, pp. 3–25). The results of an investigation to determine the optimum conditions for producing single spot welds in 20, 18, 16, and 14 S.W.G. deep-drawing quality steel sheet, by a committee of the British Welding Research Association, are reported.—R. A. R.

Flash Welding of Concentrated Areas up to 24 sq. in. in S.A.E. 1020, NE 9440 and NE 8620 Steels. D. B. Johnston. (Welding Journal, 1947, vol. 26, Feb., pp. 65-s–80-s). Details are given of the techniques used and the results obtained in experimental flash welding of 3½-, 4½-, and 5½-in. rounds of steel S.A.E. 1020, 4½-in. rounds of steel N.E. 9440, and 4-in. squares of steel N.E. 8620. It is concluded that the flash-welding of large areas is quite feasible.—C. O.

Electronic Control of A.C. Resistance Welding Machines. B. G. Higgins. (Machinist, 1947, vol. 91, May 31, pp. 137–141 ; June 7, pp. 171–175). Applications of the thyratron and ignition valves in controlling the timing, phase movement, and current of A.C. resistance welding machines are illustrated.—C. O.

Three-Phase Balanced Load Resistance Welding Machines. J. L. Solomon. (Welding Journal, 1947, vol. 26, May, pp. 426–430). Methods of reducing the peak current demands and improving the power-factor characteristics of three-phase resistance-welding machines are described and compared.—C. O.

Chain Welding. J. F. Davies. (Electrical Review, 1947, vol. 140, May 16, pp. 809–810). Details are given of the process and equipment for the electric welding of chain links during manufacture.—J. R.

Automatic Welding in Steel Plant Maintenance. H. E. Holman. (Iron and Steel Engineer, 1947, vol. 24, Apr., pp. 89–93). An automatic welding process used in the reclamation of worn steelworks equipment is described, and some of its applications and the results obtained are discussed and illustrated.—J. R.

Arc Welding of Cast Iron. T. E. Kihlgren and L. C. Minard. (American Foundrymen's Association, 1947, Preprint No. 47-54). General considerations involved in the arc-welding of cast iron with nickel electrodes are discussed. The effect of such welding variables as pre-heat, superimposition of beads in multipass welds, and bead sequence, on the microstructure, machinability, and tensile properties of welded cast iron is shown. A preliminary torch "degassing" procedure is described for improving the welding response of castings containing dissolved gas. Several examples of field applications of arc-welding of cast iron covering salvage of defective castings, repair welding, and fabrication welding are illustrated.—R. A. R.

Cast Iron Weld Repair Methods. W. J. Pfander. (American Foundryman, 1947, vol. 11, Feb., pp. 24–34). An account, with many illustrations, is given of the methods developed by the Ford Motor Company for repairing defective cast-iron "V-8" cylinder blocks by welding.—C. O.

Salvaging Iron Castings with Machinable Arc Welds. D. W. DeArmand and S. Epstein. (Foundry, 1947, vol. 75, May, pp. 146–148, 254 ; Materials and Methods, 1947, vol. 25, May, pp. 68–72). The repair of iron castings by depositing layers of weld metal and subsequently machining is illustrated.—C. O.

The Classification and Recording of Spot-Weld Defects Revealed by Radiography. R. C. McMaster, F. C. Lindvall, and Edythe Dial. (Welding Journal, 1947, vol. 26, Feb., pp. 121–128). A method for classifying the presence and extent of spot-weld defects revealed by radiography is presented ; it enables observations from radiographs to be recorded and transmitted, thus facilitating the setting-up of standards and acceptance specifications. —C. O.

Causes and Prevention of Welding Defects. F. S. Dever. (Iron Age, 1947, vol 159, Apr. 17, pp. 50–53). *See* Journ. I and S.I., 1947, vol. 156, July, p.439—R. A. R.

Cold Cracking in the Heat-Affected Zone. C. B. Voldrich. (Welding Journal, 1947, vol. 26, Mar., pp. 153-s–169-s). The nature of cold-cracking in the heat-affected base metal of metal-arc welds in steels, and the influence of the properties of the steel and of hydrogen from the electrodes on the incidence of cold-cracking are discussed. Data obtained in investigations on the evolution of hydrogen from various types of electrodes and on the influence on cracking of chemical composition and the distribution of carbon and alloying elements in steel are presented. It is concluded that cold-cracking can be prevented or minimized by controlling the hydrogen concentration in the arc atmosphere, the composition of the welding electrodes, and the rate of cooling of the weld. A list of 28 references is appended.—C. O.

Mechanical Properties and Microstructure of Open-Butt Oxyacetylene Compression Welds. H. H. Chiswick. (Welding Journal, 1947, vol. 26, Feb., pp. 115-s–119-s). A description is given of the investigation of the microstructure and mechanical properties of welds made in $\frac{1}{2} \times 1\frac{1}{4} \times 3$-in. specimens of S.A.E. 1020, N.E. 8630, and 0·72%-carbon steels by the open-butt oxy-acetylene compression-welding method. Satisfactory welds can be obtained by this method provided that the composition of the flames is properly controlled.—C. O.

Equipment for Use with Oxygen and Fuel Gases for Welding and Cutting. (British Intelligence Objectives Sub-Committee, 1947, F.I.A.T. Final Report No. 532 : H.M. Stationery Office). Items of special interest which are described are the development of machines for cutting out large shapes, the magnitude of flame-hardening applications, and under-water cutting equipment which can be used at depths unsuitable for the oxy-acetylene blowpipe.—C. O.

Oxygen Cutting of Steel at Elevated Temperatures. J. F. Kiernan and J. S. Sohn. (Welding Journal, 1947, vol. 26, Apr., pp. 301–303). Oxygen cutting of steels at temperatures in the 1400–2200° F. range is described with data on the consumption of oxygen and the cutting speeds for different thicknesses.—R. A. R.

Powder-Cutting and Scarfing of Oxidation-Resistant Materials. D. H. Fleming, jun. (Welding Journal, 1947, vol. 26, Mar., pp. 201–208). A detailed illustrated account is given of the use of iron-rich powders in the flames of oxy-acetylene torches to make possible the cutting of materials, such as stainless steels, in which one of the constituents forms a refractory oxide on combustion. The burning of the iron powder causes an intense liberation of heat, and the iron oxides formed exert a fluxing action on the other oxides present. The main applications of the process are in scarfing ingots, slabs, and billets, the removal of hot tops from stainless steel ingots and of risers from castings, the cutting of plates for cladding, and in handling scrap.—C. O.

Stainless Steel Cutting. H. G. Hughey. (Welding Journal, 1947, vol. 26, May, pp. 393–399). The development of the modern methods of flame-cutting stainless steel is described and illustrated.—C. O.

CLEANING AND PICKLING

Metal Cleaning—Methods and Results. J. R. Ewing. (Steel, 1947, vol. 120, May 5, pp. 100–101, 140–144). The part played by alkalis, liquid hydrocarbons, chlorinated solvent vapours and emulsions of hydrocarbon grease solvents and water in the cleaning of metal surfaces is discussed.—R. A. R.

Proper Surface Preparations Assure Durable Finishes. R. E. Gwyther. (Product Engineering, 1947, vol. 18, Mar., pp. 153–155). Methods of cleaning the surfaces of ferrous and non-ferrous metals before the application of paints are briefly described. Reference is made to phosphate-coating as a base for paints on iron and steel.—C. O.

Electrolytic Polishing. H. Evans and E. H. Lloyd. (Metal Industry, 1947, vol. 71, July 18, pp. 51–52). **The Electrolytic Polishing of 18/8 Stainless Steel and Nickel Silver.** H. Evans and E. H. Lloyd. (Journal of the Electrodepositors' Technical Society, 1947, vol. 22, pp. 73–84). A report is made on a number of experiments on the electrolytic polishing of 18/8 stainless-steel and nickel-silver cutlery in various electrolytes over a range of temperatures and current densities. The highest lustre on the stainless steel was obtained with an electrolyte consisting of 37% (by volume) of orthophosphoric acid, 56% of glycerine, and 7% of water, at a temperature of 100–120° C. and a current density of 5 amp./sq. in., in a polishing time of 5–10 min.—C.O.

A Surface Active Agent for the Cleaning of Metals. O. M. Morgan. (Monthly Review of the American Electroplaters' Society, 1947, Apr., pp. 430–441). A synthetic organic detergent of the alkyl-aryl sodium sulphonate type, known as "Nacconol NR," is shown to cause a definite improvement in the metal-cleaning efficiencies of alkalis and acids. A new quantitative method for evaluating metal-cleaning compounds is described.—R. A. R.

Some Observations on Chipping and Scarfing Operations. R. D. Hindson. (Blast Furnace and Steel Plant, 1947, vol. 35, Apr., pp. 439–447 ; May, pp. 553–557). In the first part of the paper, pickling, defects in steel, and chipping processes and costs are discussed. Hand-scarfing operations, equipment, and costs are dealt with in Part II, and chipping and hand-scarfing processes are compared. Inspection methods, deseamers, and mechanical-chipping processes are described.—J. R.

Preweld Cleaning. W. J. Campbell. (Steel, 1947, vol. 120, Apr. 7, pp. 96, 138–142). The mechanical and chemical methods in common use for cleaning metals before welding are reviewed.—C. O.

Submerged Combustion in Industry. W. G. See. (Iron and Steel Engineer, 1947, vol. 24, Mar., pp. 90–95). The submerged-combustion system for the pickling of steel is described, and it is claimed that a saving of approximately 50% in present pickling costs can be achieved by its use. The savings are attributed to (*a*) better agitation, (*b*) the absence of dilution, and (*c*) much faster pickling.—J. R.

Scale Removal and Surface Preparation with Sodium Hydride. H. L. Alexander. (Iron and Steel Engineer, 1947, vol. 24, May, pp. 44–51). An account is given of the development, chemistry, and applications of the sodium hydride descaling process.—C. O.

Effect of Water Conditioning on Metal Finishing Operations. R. S. Herwig. (Iron Age, 1947, vol. 159, Mar. 27, pp. 48–52). Investigations to determine the effect of using (*a*) hard water, (*b*) soft water, and (*c*) demineralized water in the washing baths in pickling and plating operations are described. The staining of parts plated in non-alkaline baths is not a function of the hardness of the water, but is related to the total solid content of the rinsing water.—R. A. R.

Quick Guide to Solutions for Stripping Metal Overlays. J. B. Mohler. (Iron Age, 1947, vol. 159, May 8, pp. 66–67). A guide to the selection of solutions for removing plated coatings from steel and other metals is presented.—R. A. R.

Electroplating and Cathodic Pickling as Causes of Hydrogen Embrittlement. C. A. Zapffe and M. Eleanor Haslem. (Wire and Wire Products, 1947, vol. 22, May, pp. 351–356, 379–381). A short description is given of a constant-rate single-bend test devised to evaluate

the hydrogen embrittlement of steel-wire specimens. This test has been used for (1) determining the effect of pickling time and temperature on the degree of embrittlement during cathodic pickling, (2) proving that hydrogen absorption during pickling is the same for both alkaline and acid electrolytes, (3) showing the effect of arsenic and sulphur in increasing pickling embrittlement, and (4) measuring the embrittlement caused by chromium and cadmium plating.—C. O.

The Electrolytic Polishing of Metals in Research and Industry. R. W. K. Honeycombe and D. S. Kemsley. (Melbourne University, Council for Scientific and Industrial Research, 1947, Serial No. 150, Physical Metallurgy Report No. 3). The phenomenon of electrolytic polishing is described and the theory is briefly discussed. Methods used in the laboratory and in industry are reviewed with data on the composition of the electrolytes. There are 62 references.—R. A. R.

Lime Treatment of Waste Pickle Liquor. C. J. Lewis. (Iron Age, 1947, vol. 159, June 26, pp. 45–49). The relative merits of limestone, quicklime, and slaked lime as neutralizing agents for the treatment of waste pickling liquors are discussed. Methods of preparing and using these materials and techniques for disposing of the sludge are described.—C. O.

Disposal of Waste Pickle Liquors May Be Facilitated by Mellon-Developed Process. R. D. Hoak. (American Chemical Society : Steel, 1947, vol. 120, May 12, p. 121). A process has been developed in the laboratories of the Mellon Institute by which the sulphate ion in waste pickling liquor can be combined with the ammonia produced from coke-oven gas to make ammonium sulphate.—R. A. R.

Waste Pickle Liquor. R. D. Hoak. (Industrial and Engineering Chemistry, 1947, vol. 39, May 15, pp. 614–618). The most important processes for the treatment of pickle liquor are outlined with numerous references to the literature, and brief descriptions are given of some processes developed on a laboratory scale at the Mellon Institute by the American Iron and Steel Institute.—D. R. S.

Lime Treatment of Waste Pickle Liquor. R. D. Hoak, C. J. Lewis, C. J. Sindlinger, and B. Klein. (Industrial and Engineering Chemistry, 1947, vol. 39, Feb., pp. 131–135). Dolomitic lime was found to be as effective as high-calcium lime in treating waste pickle liquor; the lower rate of reaction and the greater bulk of the sludge formed were, however, disadvantageous in certain cases.—D. R. S.

PROTECTIVE COATINGS

Electro-Plating—The Periodic Reverse Current Process. (Automobile Engineer, 1947, vol. 37, June, pp. 227–229). An outline is given of the periodic-reverse-current process for electroplating metals. During each cycle of this process a plating current is applied for a period of from 2 to 40 sec. to deposit a layer of metal, after which the current is reversed for 0·5 to 5 sec. to remove unsound or inferior metal. The surface is thus progessively smoothed and polished, and, because of the soundness of the deposit, adhesion is increased. The absence of polarization enables high current densities and hence high plating speeds to be used. The cost of the process for the production of highly polished finishes compares favourably with that of direct-current plating and hand buffing and polishing.—C. O.

Periodic Reverse Current Electro Plating. G. W. Jernstedt. (Steel, 1947, vol. 120, Apr. 28, pp. 100–102, 134). It is claimed that in the plating of copper, nickel, and chromium, a better finished is obtained when the plating current is reversed at short intervals during the process. Methods of doing this and the results obtained are discussed.—R. A. R.

Control Apparatus for the Production of Uniform Electro-Deposits from a Rectified A.C. Supply. D. Ashby and S. Wernick. (Monthly Review of the American Electroplaters' Society, 1947, Jan., pp. 42–49). Descriptions are given of control apparatus for applying and maintaining a given current throughout the deposition period in electroplating, and for cutting off the current when the process time is completed.—R. A. R.

Practical Methods in Heavy Industrial Nickel Plating. E. J. Roehl. (Metal Finishing, 1947, vol. 45, May, pp. 56–59, 71). The techniques for electrodepositing thick coatings of nickel on to iron and steel surfaces are described.—C. O.

A New Zinc-Base Finish for Steel Parts. J. A. Williams. (Materials and Methods, 1947, vol. 25, Mar., pp. 95–96). A description is given of the "Anozinc Clear" plating treatment, and the final lacquering and baking, in the preparation of steel surfaces to give a zinc-base finish of bright metallic appearance. Applications, including the coating of refrigerator shelves, are mentioned.—R. A. R.

Continuous Hot Galvanizing of Strip. E. A. Matteson. (Iron and Steel Engineer, 1947, vol. 24, Mar., pp. 61–62). Methods for the continuous hot-galvanizing of strip are discussed, and it is concluded that the highly adherent coatings made possible by new methods should create new markets for galvanized material.—J. R.

Zinc Plating for Corrosion Resistance and Decorative Finishing. W. F. Coxon. (Metal Treatment, 1947, vol. 14, Spring Issue, pp. 38–40). The developments in recent years in the use of zinc plating for the protection of steel, either alone or as a base for chromium plating, and for decorative finishing are reviewed.—C. O.

The Attack of Molten Zinc on Steel in Hot-Dip Galvanising. H. Bablik. (Metal Treatment, 1947, vol. 14, Spring Issue, pp. 29–35). The attack of molten zinc on steel and the relationship of the coatings formed to the pre-existing structure of the steel are discussed. It is shown that the coatings are made up of layers corresponding to the homogeneous phase fields in the iron–zinc equilibrium diagram, and that some simple relations exist between the thickness of these layers. Certain analogies are found between the attack on steel by molten zinc and by sulphuric acid.—R. A. R.

Specifications and Tests for Electrodeposited Metallic Coatings. (American Society for Testing Materials and American Electroplaters' Society, Nov., 1946, Pamphlet). Tentative specifications are given for electrodeposited coatings of zinc, cadmium, and lead on steel, and for chromate finishes on electrodeposited zinc, hot-dipped galvanized, and zinc die-cast surfaces. Practices for chromium-plating steel for engineering use and for preparing low-carbon steel for electroplating are recommended, and descriptions are given of tests for the local thickness of electrodeposited coatings and of a salt-spray test.—C. O.

Tin Plating and Control of Tin Solution by Anode Colour. J. F. Daymude. (Products Finishing, 1947, vol. 11, Mar., pp. 42–56). Detailed instructions and data for electrotinning practice are given, and a method for the visual control of the solutions, based on anode colour, is described.—J. R.

Fusing of Electrodeposited Tin Coatings. J. Falk. (Metal Finishing, 1947, vol. 45, Apr., pp. 63–65, 71). The production of extremely lustrous tin finishes by fusing electrodeposited tin coatings less than 0·0005 in. in thickness in immersion-heated oil baths is described.—C. O.

Gas-Heated Ovens and Tin-Flowing Unit Featured in Crown Cork and Seal Plant. (Industrial Heating, 1947, vol. 14, Mar., pp. 449–460). A short description is given of the equipment used by the Crown Seal and Cork Company, Baltimore, in the production of cork-lined metal bottle-caps. The low-carbon rimming-steel strip for the caps is tinned electrolytically and passed continuously between gas-fired radiant-heat burners. The tin coating is fused and after quenching and drying has a very bright finish.—C. O.

Tin as a Coating Material in the Wire Industry. B. W. Gonser. (Wire and Wire Products, 1947, vol. 22. Mar., pp. 207–210, 242–243). Brief accounts are given of the methods of coating wire with tin by hot-dipping, electrolysis, molten-salt processes, deposition from vapours, and spraying.—C. O.

Tin Coating Meehanite. (Iron Age, 1947, vol. 159, June 26, p. 57). The tin-coating of Meehanite cast iron by the fused chloride and fused nitrate methods is described very briefly.—C. O.

Three Scuff-Resistant Coatings for Ferrous Wearing Surfaces. F. C. Young and B. B. Davis. (S.A.E. Journal, 1947, vol. 55, May, pp. 42–43). The applications of iron manganese phosphate, iron oxide, and caustic sulphur (an aqueous solution of 50% of sodium hydroxide and 1% of powdered sulphur) for protecting metal surfaces from wear are discussed briefly.—C. O.

The Effect of the Basis Metal on the Electrodeposition of Brass. W. D. Rae. (Journal of the Electrodepositors' Technical Society, 1947, vol. 22, pp. 85–96). Measurements of potential were made during the brass-plating of mild steel and cast iron, and the plating was examined metallographically and by X-ray diffraction to determine the influence of the base metal on the type and structure of the brass deposited. It is concluded that normally brass plating is unaffected by the base metal, assuming its normal structure a short time after the commencement of plating. Physical or chemical non-uniformity of the base metal may, however, modify the plating considerably.—C. O.

Electrodeposited Silver on Steel for Glass-to-Metal Seals. N. S. Freedman. (Electrochemical Society, 1947, Preprint No. 91-19). The silver-plating of steel for the production of glass-to-metal disc-type seals for electron tubes is described.—D. R. S.

Barrel Plated Multiple Coatings. M. Mazzone and F. McKnight. (Metal Finishing, 1947, vol. 45, June, pp. 81–85). A detailed account is given of the electroplating practice of a leading American toy-train manufacturer. Most of the plating is done in revolving barrels made of rubber or laminated resin, in which the small parts are able to tumble freely, electrical contact being maintained by flexible "danglers."—C. O.

Electroplating Shop Costing. A. W. Wallbank. (Journal of the Electrodepositors' Technical Society, 1947, vol. 22, pp. 97–117). The commonly used methods of calculating the cost of electroplating are criticized. An alternative method is suggested whereby the total costs of deposition are separated from all other plating-shop expenses. Applications of the method are described.—C. O.

Metal Spraying Processes. (Machinist, 1947, vol. 91, May 24, pp. 101–104). Typical wire-, powder-, and molten-metal spraying guns are illustrated. British and American practices in metal spraying are compared. In general the Americans favour the use of higher rates of deposition.—C. O.

Surface Preparation for Metal Spraying. (Machinist, 1947, vol. 91, June 21, pp. 235–238). The methods commonly used for preparing surfaces for metal spraying are described. In all of them the object is to obtain a roughened surface, since adhesion is by mechanical interlocking rather than alloying. The porosity of sprayed coatings and galvanic action are also briefly discussed.—C. O.

Some Special Applications of Flame Spraying. (Machinist, 1947, vol. 91, July 5, pp. 299–302). The application of metal spraying for the coating of rubber and plastics, and for the protection of metals by processes such as Aluminizing is described.—C. O.

Ceramic Coatings for High-Temperature Protection of Steel. W. N. Harrison, D. G. Moore, and J. C. Richmond. (Journal of Research of the National Bureau of Standards, 1947, vol. 38, Mar., pp. 293–307). The testing of ceramic coatings during the development of coatings for the protection of low carbon steels in service at high temperatures is described in detail. A layer 0·002–0·003 in. thick of a mixture of a special grade of calcined alumina and a conventional type of ground-coat frit, ground in water and applied and fired by the usual methods proved much superior to conventional enamels in (*a*) resistance to severe thermal shock, (*b*) protection of metal against oxidation by air at 1250° F., and (*c*) freedom from cracking and blistering.—C. O.

Electrostatic Spraying of Porcelain Enamel. R. E. Helmuth. (Steel Processing, 1947, vol. 33, May, pp. 281–284). The equipment for spraying metal articles with porcelain enamel by a process in which the particles of enamel receive a negative charge in an electrostatic field and the articles are positively charged is described and illustrated, and the advantages of the process are discussed.—R. A. R.

Spraying in Electrostatic Zones. J. Parina, jun. (Steel, 1947, vol. 120, May 12, pp. 106–107, 139–144). A description is given of the equipment and process for electrostatic paint spraying of metal articles. In this process the paint spray and the article receive static charges of opposite polarity so that the paint is attracted on to the metal surface and little paint is wasted.—R. A. R.

For Quality We Chose Modern Finishing Methods. L. A. Brown. (Machinist, 1947, vol. 91, June 7, pp. 177–181). An illustrated account is given of a modern plant for coating household articles with porcelain and synthetic enamels.—C. O.

Testing Resistance of Enameled Surfaces to Scratching, Gouging, and Abrasion. F. A. Petersen. (Journal of the American Ceramic Society, 1947, vol. 30, Mar. 1, pp. 94–104). Seven types of cover-coat enamels were tested for resistance to scratching, gouging, and abrasion. Chemical durability and reflecting power were found to have little or no correlation with the resistance of the enamels to abrasion. The bubble structure of the enamel layer seemed to have the greatest effect on the abrasion and scratch resistance of the enamel.—C. O.

Phosphating Process in Germany for the Surface Treatment of Iron and Steel. (British Intelligence Objectives Sub-Committee, 1947, Final Report No. 1298 : H.M. Stationery Office). An account is given of the size and scope of the industries dealing with the phosphatizing of iron and steel surfaces in Germany. Features of special note were : (*a*) The widespread use of phosphatizing for rust prevention ; (*b*) phosphatizing to facilitate cold-shaping ; (*c*) the developments of cold-phosphatizing methods ; and (*d*) the phosphatizing of welded steel food cans before lacquering.—R. A. R.

New Phosphating Tunnels Installed at Ford. H. Chase. (Iron Age, 1947, vol. 159, Apr. 10, pp. 82–84). A short account is given of two tunnels recently built by the Ford Motor Company for the phosphate spraying of sheet-steel motor-car parts.—C. O.

Chemically Generated Film Aids in Corrosion Control. (Steel, 1947, vol. 120, June 2, pp. 101–102, 144). A short account is given of the properties and applications of Iridite, a complex chromate film which can be formed on zinc and cadmium surfaces by immersion in a patent solution. The film may be cold-worked and dyed in various colours. It confers high corrosion resistance and may be used as a base for paints.—C. O.

The Falling Sand Abrasion Tester. C. C. Hipkins and R. J. Phair. (A.S.T.M. Bulletin, 1946, Dec., pp. 18–22). An account is given of a method for determining the abrasion resistance of organic coatings on metals by measuring the amount of sand falling from a definite height required to expose a small area of the base metal. —C. O.

Electrical Properties of Paint Films on Metals. F. Wormwell and D. M. Brasher. (Nature, 1947, vol. 159, May 17, pp. 678–679). As part of an investigation on the protective properties of paint films on metals exposed to sea water a study has been made of the electrical characteristics (potential, resistance, and capacity) of such films on specimens immersed in artificial sea water, with particular reference to the variations in these characteristics with increasing corrosion. The results obtained indicate that the paint film behaves like a "leaking condenser."—D. R. S.

Inorganic Pigments—Synthetic Iron Oxides. (British Intelligence Objectives Sub-Committee, 1947, Final Report No. 1272 : H.M. Stationery Office). A detailed account is given of the methods used at six German plants producing yellow, red, and black oxides of iron for pigments. Production was on a very large scale, and at the I.G. Farbenindustrie works at Uerdingen the output at one time reached 3000 tons per month, largely by means of a process in which yellow and black oxides were formed from cast iron or carbon steel during the reduction of nitrobenzene to aniline. —C. O.

PROPERTIES AND TESTS

Mechanical Testing Procedures and Definitions For Plates and Structural Sections. (American Iron and Steel Institute, Nov., 1942, Contributions to the Metallurgy of Steel, No. 16).—R. A. R.

The Value of Highly Destructive Testing. M. G. Corson. (Metal Progress, 1947, vol. 51, Apr., pp. 613–616). Two examples are given to illustrate the importance of destructive testing of machine parts in improving design and reducing production costs. Parts of variable-pitch propellers, rejected by magnetic testing, were found to have considerably better tensile properties than were called for by the specifications or working requirements.—C. O.

Malleable Iron Test Bars. J. E. Rehder. (American Foundrymen's Association : Canadian Metals and Metallurgical Industries, 1947, vol. 10, Feb., pp. 14–17). The relationship between the properties of metals as determined from tensile test-bars and the performance of castings under service conditions is discussed. The relative values of the physical properties of malleable iron are considered from the viewpoint of the use of the metal in various engineering components.—C. O.

The Velocity Aspect of Tension-Impact Testing. W. H. Hoppmann, II. (Bureau of Ships Metallurgical Conference : American Society for Testing Materials, 1947, Preprint No. 32). The work done by various investigators on the influence of velocity on tension-impact testing is briefly reviewed, and a description is given of the "guillotine" impact-testing machine used at the United States Naval Shipyard, Brooklyn, for the study of the effect of moderately high velocities on the impact resistance of materials. Von Kármán's theory of the propagation of plastic strains and his concept of "critical velocity" are outlined, and applied to the calculation of the critical velocity for hard-drawn copper. The result has been verified experimentally using the guillotine machine.—C. O.

Steel in Tension. A. C. Vivian. (Structural Engineer, 1947, vol. 25, May, pp. 165–178). The distinction between elastic and plastic deformation is explained and stress-strain formulæ developed for both cases. Particular reference is made to the case of steel under tensile stress, and data are given for tensile tests on a mild steel flat bar. The work of F. C. Lea on stress-strain relationships for steel is quoted. It is suggested that the "effective ductility" of steel as measured by its strain at the ultimate tensile stress is of greater significance than its percentage elongation.—M. A. V.

The Speed of Propagation of Brittle Cracks in Steel. G. Hudson and M. Greenfield. (Journal of Applied Physics, 1947, vol. 18, Apr., pp. 405–407). A description is given of the electronic timing apparatus used to determine the speed of propagation of a brittle failure in a notched medium-carbon steel plate. The speed was approximately 40,400 in./sec.—C. O.

Effect of Prior Tensile Strain on Fracture. E. Saibel. (American Institute of Mining and Metallurgical Engineers, Technical Publication No. 2186 : Metals Technology 1947, vol. 14, June). A thermodynamical theory of the effect of previous strain on the fracture stress of a metal is outlined and compared with experimental findings.—C. O.

Residual Stresses in Welded Structures. W. M. Wilson. and C. C. Hao. (Welding Journal, 1947, vol. 26, May, pp. 295-s–320-s). The magnitude of the thermal stresses caused by welding and the strength and ductility of welds have been investigated. The residual stresses in longitudinal butt welds in low-carbon steel plates were determined by relaxation methods; the weld metal and the heat-affected base metal were tested in fatigue and tension. The residual stresses in the weld and heat-affected base metal were approximately equal to the yield point of the unaffected base metal. A narrow strip containing the weld had a much higher yield point, higher ultimate strength, and lower ductility than the unaffected metal.

Tests were also made on plates with circular welded seams, from which it was found that the nearly equal diaxial stresses in the weld and the area enclosed by the weld were of the order of the yield point of the unaffected metal.—C. O.

An Experimental Study of the Propagation of Plastic Deformation under Conditions of Longitudinal Impact. P. E. Duwez and D. S. Clark. (American Society for Testing Materials, 1947, Preprint No. 31). Von Kármán's theory of the propagation of plastic strain is reviewed and experimental evidence of the validity of the theory as applied to longitudinal impact is presented. The special "sling shot" and rotary impact machines used for tests in tension and compression on long wires and rods of 8-in. gauge length are described.—C. O.

Intercrystalline Cohesion and the Stress-Rupture Test. H. H. Bleakney. (American Society for Testing Materials, 1947, Preprint No. 34). The existing theories of intercrystalline cracking are outlined and criticized in the light of all the evidence which the author has been able to collect on the subject. A hypothesis is suggested to account for certain kinds of failure. On this hypothesis a programme of investigation has been built to attempt to answer certain questions raised by the evidence.—C. O.

Five Theories of Static Failure Explained and Compared. C. Lipson. (Product Engineering, 1947, vol. 18, June, pp. 157–160). The principles of the maximum-stress, maximum-shear, maximum-strain, strain-energy, and constant-energy-of-distortion theories of the failure of metals under static loads are briefly explained. The theories are compared in their application to the fatigue loading of cast iron and mild steel—C. O.

X-Ray Diffraction Studies of Yielding in Mild Steel. J. M. Cowley and M. S. Paterson. (Nature, 1947, vol. 159, June 21, p. 846). A normalized 0·26% carbon steel bar was stretched in a tensile testing machine until the Lüders lines appeared over part of its length, and the distribution of plastic deformation was studied by X-ray diffraction, the back-reflection technique with cobalt $K\alpha$ radiation being used. Plastic deformation was indicated by a blurring of the spots of the diffraction pattern as the individual crystals giving the reflections became distorted. The results obtained showed that the amount of plastic deformation is much greater between the Lüders lines formed at an early stage of stretching than between those formed later, and is very slight between the most recently formed lines. A picture of the mode of yielding in mild steel ensuing from these results is briefly discussed. —D. R. S.

Symposium on the Failure of Metals by Fatigue. (University of Melbourne, Dec., 1946). A Symposium on "The Failure of Metals by Fatigue" was held in December, 1946, under the auspices of the Faculty of Engineering, Melbourne University. Abstracts of some of the papers are given below.—R. A. R.

The Failure of Metals by Fatigue. J. N. Greenwood. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 1). This paper forms a general introduction to the subject. After a brief historical survey the physical features of a fatigue failure are described. The relationship between magnitude of stress and number of cycles, the presence of stress concentration, and the influence of repeated stress below and above the fatigue limit are discussed. Finally, the various types of stress concentrator are listed.

Metallography, Fatigue of Metals and Conventional Stress Analysis. H. F. Moore. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 2). The author outlines the history of stress analysis in pre-metallographic days. Failure of parts designed on this basis led Wöhler to his experimental investigation of the effects of repeated stresses. The use of the microscope by Ewing, Rosenhain and Humphrey revealed the slip nature of mechanical deformation in the crystals of which metals are built up. The microscope further revealed that "fatigue" is a spreading crack, starting at some spot where the localized stresses are higher than average. The influence of stress concentration and the sensitivity of materials to notches are considered. Engineers are reminded of the limitations of stress analysis.

Theories of the Mechanism of Fatigue Failure. W. Boas. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 3). The development of ideas on the mechanism of fatigue failure is described. Emphasis is laid on the experimental results on which any theory has to be based. During the last twenty years experiments carried out on metallic single crystals and those using X-ray diffraction methods have changed the aspect of the problem. Failure by fatigue is merely a special case of failure under all forms of stressing actions and not a special problem of its own, and it must be considered as a consequence of slip. The deformation is localized in the immediate neighbourhood of the fatigue fracture where it reaches a critical amount. It is the total strain which has a certain characteristic value at fracture. Finally, recent theories of the mechanism of fatigue failure are described.

Repeated Loading on Structures. A. G. Pugsley. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 6). The discussion is restricted to the consideration of loads which are repeated at intervals ranging from seconds to hours and at rates which can be regarded as gradual compared with the natural periods of the structures concerned. Such a definition is appropriate to the varying loads occurring in the structure of an aeroplane in flight, as a result of deliberate manoeuvres or of encountering gusts. The magnitude and frequencies of the fluctuating loads occurring in flight, as determined by the statistical analysis of flight load records, and their influence on structural failures in the air are briefly discussed. Joints are the chief source of weakness under repeated loading owing to the presence of stress concentrations and it is important to carry repeated-load tests to ultimate collapse since initial failure may not give a true indication of the mode of ultimate failure.

Structures Liable to Fatigue Failure and Some Considerations in Their Design. L. R. Jackson and H. J. Grover. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, (Preprint No. 7). Results of experimental work recently carried out in the United States are reviewed and a number of the more important considerations such as stress distribution under repeated loads, types of repeated stress, stress concentration, size effect and surface conditions are discussed. Estimates of the fatigue life of many structures are limited largely by lack of detailed knowledge of service stresses (and other conditions conducive to decreased fatigue strength). As such knowledge increases some gaps in present information about the fatigue properties of materials may become more significant.

The Electrochemistry of Corrosion Fatigue. U. R. Evans. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 8). Alternating stress and corrosive influences, operating together, produce damage more quickly than if they operate one at a time. The electrochemical mechanism of stressless corrosion has been quantitatively established by four independent methods. Electrochemical processes are often more destructive than direct chemical combination, because they frequently yield soluble substances as primary corrosion products, so that stifling does not in general occur. Corrosion fatigue also appears to follow an electrochemical course and in recent years electrochemical methods have been used in its study. If a wire subjected to alternating stress is kept wet with a mixture of potassium chromate and chloride, the electrode potential suddenly drops at the moment when the protective film formed by the chromate breaks down. It has been found that in general the "film-life," as well as the "wire-life" (the period which elapses before breakage), is shortened by increase of stress range and of chloride concentration, but that both are lengthened by an increase of chromate concentration.

Winding a steel specimen with zinc tape gives cathodic protection and prolongs the life, even though the access of corrosive liquid to the steel is not prevented. Another method of increasing corrosion-

fatigue life depends on application of paint so richly pigmented with zinc dust that the coat constitutes a conducting mass. Paints have been produced which, after drying, yield a film containing 95% by weight of metallic zinc. Such films are not, in general, impervious, and the protection is largely electrochemical. Experiments to ascertain whether contact with zinc might in some circumstances shorten the life by introducing hydrogen and thus causing embrittlement gave reassuring results; no dangerous embrittlement was obtained by introducing hydrogen into steel, provided that the stress did not exceed the yield point. Other experiments in which cathodic currents were applied to a wire undergoing corrosion-fatigue in a potassium chloride solution indicated that, as the cathodic current applied was increased, the rate of production of iron compounds steadily diminished, finally becoming undetectable. The number of cracks produced also decreased with increase of cathodic current, finally becoming zero. The life of the wire at first diminished slightly as the cathodic current was increased, but then increased again, finally becoming very long. Owing to stress concentration, a few cracks may be more weakening than many. The results quoted point to the possibility of obtaining complete protection by means of a cathodic current, but the value of the current needed increases with the stress range.

A picture of the mechanism of corrosion fatigue based on the new experimental results is suggested.

The Detection of Fatigue Cracks. C. W. Orr. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 9). There are no known methods of testing which will predict damage by fatigue before a crack develops. The various methods and equipment used for crack detection in both ferrous and non-ferrous materials are given. Magnetic-particle, fluid-penetration, radiographic, etching, lacquer-coating, eddy-current and sonic methods are described and the advantages and limitations of each discussed. Crack propagation and the question of when to inspect, and when to reject because of fatigue cracks are briefly discussed.

The Measurement of Dynamic Strain. F. W. Hooton. (Symposium on the Failure of Metals by Fatigue, University of Melbourne, Dec., 1946, Preprint No. 10). The requirements of devices for measuring dynamic strains are briefly discussed and the general principles of operation of various types of electrical and mechanical gauges are described. A detailed consideration of the electrical resistance strain gauge and its associated equipment is made.

The additional complications introduced when strains are to be measured on rotating members are outlined. (Further abstracts of the Symposium will be published later).

Tool Steels. L. Sanderson. (British Steelmaker, 1947, vol. 13, Jan., pp. 34–37; Feb., pp. 90–93; Mar., pp. 147–150; Apr., pp. 192–195; May, pp. 246–249; June, pp. 294–297; July, pp. 350–355). In Part I the system of classification of tool steels is briefly explained. Part II deals with hot-working steels, die steels, and shock-resistant steels (chisels and riveting steels). In Parts III–VI high-speed steels, special alloy tool steels, and their applications are discussed, and Part VII is concerned with the grinding, heat-treatment, and quenching of tools.—J. R.

Torsion Bar Springs. (Metal Progress, 1947, vol. 51, May, pp. 771–774). The principles of "torsion-bar suspension" are explained and a short account is given of the properties of an alloy steel used by an American company for making torsion bars.—C. O.

Composite Materials. (Steel, 1947, vol. 120, June 9, pp. 76–79, 98–101). A general account is given of the properties and applications of sheet materials made from laminations of ferrous and non-ferrous metals, wood, and plastics.—C. O.

The Development of Steel Kegs. (American Iron and Steel Institute, May, 1946, Contributions to the Metallurgy of Steel, No. 13). A report is presented on investigations of the possibilities of designing and using steel kegs for the shipment of nails, spikes, bolts, staples, etc., customarily shipped in wooden kegs or boxes.—R. A. R.

Centres of Crystallization of Temper Carbon in Black-Heart Malleable Cast Iron. G. Joly. (Fonderie, 1946, Nov., pp. 375–382). A report is presented on work done by the Russian scientist Smirnov, who studied the frequency of temper carbon spots and their effect on the mechanical properties of malleable cast iron. The frequency is affected by overheating; adding small amounts of alloying material, *e.g.*, aluminium, magnesium, silicon, and titanium; by heat-treatment (not regarded as economical); and by the furnace atmosphere.

Tensile strength increases, and elongation decreases, with increasing number of temper carbon spots, but the difference is noticeable only with variations of the order of several hundred per square millimetre.—R. A. H.

The Dimensional Stability of a High Duty Cast Iron. L. W. Nickols. (Machinery, 1947, vol. 70, June 12, pp. 623–624). The dimensional stability of a high-duty cast iron (total C 3%, Si 1·15%, S 0·1%, P 0·08%, Mn 0·85%, Cr 0·25%) was investigated. Its high degree of stability was confirmed, except in cases where the hardening operation was not followed by a suitable tempering operation.—R. A. R.

Tracer-Point Sharpness as Affecting Roughness Measurements. D. E. Williamson. (Transactions of the American Society of Mechanical Engineers, 1947, vol. 69, May, pp. 319–323). Data are presented to show that the measurement of the average roughness on metal surfaces finished with abrasives can be satisfactorily carried out by diamond tracer-point methods. A tip radius of 0·0005 in. is adequate for any degree of smoothness.—C. O.

The Factorial Experiment in Engineering Research. M. K. Barnett. (American Institute of Mining and Metallurgical Engineers, Technical Publication No. 2161: Metals Technology, 1947, vol. 14, June). The design of experimental research into problems involving the influence of more than one variable is discussed. The "factorial" method, in which all possible combinations of the variables are subjected to tests, is explained, analysed, and criticized.—C. O.

Research in Physical Metallurgy. G. S. Farnham. (Canadian Mining and Metallurgical Bulletin, 1947, Apr., pp. 205–214). The outstanding developments in metallurgical practice in recent decades are reviewed and topics of current interest to the physical metallurgist are discussed. The organization of research work in Canada is compared with that in other countries.—C. O.

METALLOGRAPHY

Phase Contrast in the Photomicrography of Metals. F. W. Cuckow. (Nature, 1947, vol. 159, May 10, pp. 639–640). The application of the so-called phase-contrast method to the vertical-illumination metallurgical microscope is briefly described. Micrographs of alloy steels obtained in this way show details previously revealed only by the electron microscope.—D. R. S.

New Developments in Electron Microscopy. J. L. Farrant. (Electrical Engineer and Merchandiser,

1947, vol. 23, Jan. 15, pp. 315–319). Short descriptions are given of the voltage regulation and the vacuum gauges of the R.C.A. (Radio Corporation of America) Universal electron microscope, and of polystyrene–silica replica and shadow techniques for examining surface structures.—C. O.

Design and Construction of an Electron Microscope. M. E. Haine. (Engineering, 1947, vol. 164, July 4, pp. 20–24). The individual parts of the E.M.2 (Metropolitan-Vickers) electron microscope are described and illustrated.—C. O.

Preparation and Uses of Silica Replicas in Electron Microscopy. C. H. Gerould. (Journal of Applied Physics, 1947, vol. 18, Apr., pp. 333–343). A detailed description of the preparation of silica replicas and substrates and their uses is presented, with photographs and electron micrographs. A polystyrene lacquer may be used in the place of the conventional moulding in preparing replicas of specimens which cannot be subjected to the temperatures and pressures of the ordinary technique. Methods successfully used for dispersing powders and an aid to the stereoscopic inspection of replicas are also described. —C. O.

Primary Etching of Welds. E. Magnusson. (Jernkontorets Annaler, 1947, vol. 131, No. 6, pp. 212–223). [In Swedish]. Descriptions are given of methods of preparing and etching specimens of welds to enable the primary structure to be studied under the microscope. The heat-treatment and polishing procedures and a number of etching reagents are discussed. Stead's reagent is one of the best for fine structures. A preliminary etch with nitric acid should be followed by repeated etching beginning with diluted Stead's reagent. The mechanism of the etching reactions is explained by the supposition that galvanic elements are formed between iron and precipitated copper. Twenty-seven micrographs are shown.—R. A. R.

Graphitization in the Solid State and Its Industrial Application in Malleablizing. H. Laplanche. (Fonderie, 1946, Apr., pp. 129–142; May, pp. 169–179). In the first part, which deals with the primary graphitization of a pure iron–carbon alloy, the author describes the mechanism of migration of carbon atoms in an austenitic structure and the conditions of their crystallization into graphite. He also quotes C. Wells' work (*see* Journ. I. and S.I., 1938, No. I, p. 88A) in which a stable iron–graphite equilibrium diagram is superimposed upon the usual metastable iron–cementite diagram. The rate of graphitization is calculated, and special reference is made to the case where 1% of silicon is present. The equilibrium diagram from A. E. White and R. Schneidewind (*see* Journ. I. and S.I., 1934, No. II, p. 551) is reproduced.

The second part of the paper contains a metallographic study of the graphitization which occurs when carbon is deposited from the saturated solid solution on cooling, giving the "bull's eye" structure. The effect of phosphorus, sulphur, manganese and other elements on graphitization is shown.—R. A. H.

Isothermal Transformation of Molybdenum Cast Iron. C. Nagler and R. L. Dowdell. (American Foundrymen's Association, 1947, Preprint No. 47-17). The effect of alloying elements on the transformation of austenite in cast iron was reported on by C. Nagler and W. P. Wood (*see* Journ. I. and S.I., 1942, No. II, p. 186A); in this paper an investigation is described in which similar apparatus was used to study the effect of molybdenum. The results indicated that the effects of additions of up to 0·50% were: (1) To slow down the transformation rate at temperatures from 700° F. up to the lower critical point; (2) practically nil as regards the isothermal transformation rates in the 400–700° F. range; and (3) not to increase the hardenability when cast irons were isothermally transformed in the 400–700° F. range. The molybdenum cast irons investigated did not have a simple typical **S**-curve; they showed deep hardenability in the 900–1300° F. range.—R. A. R.

Thermodynamic Activities and Diffusion in Metallic Solid Solutions. C. E. Birchenall and R. F. Mehl. (American Institute of Mining and Metallurgical Engineers, Technical Publication No. 2168: Metals Technology, 1947, vol. 14, June). The process of diffusion in metallic binary solid solutions is examined in detail. It is quantitatively demonstrated that the activity gradient is more fundamental than the concentration gradient in the process of diffusion, in the cases of the copper–zinc and iron–carbon systems, and probably in general.—C. O.

An Interpretation of the Constitution of Iron–Carbon–Silicon Alloys. J. E. Rehder. (American Foundrymen's Association, 1947, Preprint No. 47-11). Review and correlation of published experimental data on the iron–carbon–silicon system are shown to lead to the conclusion that iron–carbon–silicon alloys, at least in the range of 0–4% carbon and 0–4% silicon, act as though the silicon is all present as dissolved Fe_3Si, with the iron and carbon acting as a pure iron–carbon alloy. It is suggested that similar reasoning can be applied to the low phosphorus range of iron–carbon–phosphorus alloys.—R. A. R.

CORROSION

The Corrosion of Iron and Steel and Its Prevention. J. C. Hudson. (Journal of the Oil and Colour Chemists' Association, 1947, vol. 30, Feb., pp. 35–52, Reprint). A broad general survey of the important features of the corrosion of iron and steel of interest to paint technologists is presented under the following headings: (1) The corrosion of bare iron and steel; (2) protective measures against rusting; and (3) protective coatings for (*a*) iron and steel, (*b*) heavy structural steelwork, and (*c*) light gauge iron and steel parts.—R. A. R.

Atmospheric Corrosion of Iron and Steel. F. C. Strong. (Monthly Review of the American Electroplaters' Society, 1947, May, pp. 551–556). A summary of present-day knowledge of the atmospheric corrosion of iron and steel is presented together with an outline of methods of protection.—R. A. R.

Corrosion Processes. U. R. Evans. (Metal Industry, 1947, vol. 70, May 9, pp. 335–337; May 16, pp. 355–357). A survey is made of the work of British investigators in the field of metallic corrosion.—R. E.

The Mechanism of Oxidation and Tarnishing. U. R. Evans. (Electrochemical Society, 1947, Preprint 91-5). The various modes of thickening oxide and sulphide films are considered mathematically. The outward migration of cations and electrons gives the usual parabolic film growth, but mechanical breakdowns due to compressive stresses may lead to rectilinear or logarithmic growth. Depending on the relationship between adhesion, cohesion, and compressive force, these breakdowns may be of three kinds, *viz.*, blistering, flaking, and shear-cracking. The theory developed is applied to the interpretation of experimental results obtained by various authors on non-ferrous metals. —D. R. S.

Corrosion of Metals—Metals in Aircraft Engine Cooling Systems. P. F. Thompson. (Australian Council for Aeronautics, 1946, Report ACA-24). A general account of the theory of corrosion, including the functions of the active corroding agents and its application to cooling systems of aircraft engines is given and a critical study of inhibitors and their use and abuse is made.—R.A.R.

Effect of Sulphur Bacteria on Corrosion. L. Liberthson. (Iron and Steel Engineer, 1947, vol. 24, June, pp. 69–73). Aspects of industrial microbiology which are of particular interest to the engineer, *e.g.*, the deterioration of cutting oil emulsions and the aerobic and anaerobic corrosion of metals by bacteria, are discussed in general terms.—C. O.

The Biology of Ship-Fouling Organisms. K. A. Pyefinch. (Monthly Science News, 1947, No. 2, pp. 4–6). The nature of biological settlement on ships' hulls is described and factors influencing its growth and prevention by toxic paints are discussed.—M. A. V.

The Role of Algae in Corrosion. H. C. Myers. (Journal of the American Water Works Association, 1947, vol. 39, Apr., pp. 322–324). An unusual case of the growth of oscillatoria is reported. The growth took place in open steel tanks, in untreated water of *p*H 9·4–9·5; the paint was attacked, and pitting occurred. The growth was completely stopped by covering the tanks, and thus depriving the algae of the sunlight necessary for chlorophyll formation.—M. A. V.

Final Report on German Naval Boilers, with Particular Reference to Design, Corrosion, and Boiler Feed Water Treatment. (British Intelligence Objectives Sub-Committee, 1947, Final Report No. 1333: H.M. Stationery Office). The report deals with the corrosion of German naval boilers in general, and the de-aeration of feed water in particular; it should be read in conjunction with Report No. 878 (*see* Journ. I. and S.I., 1947, vol. 155, Apr., p. 638).—R. A. R.

Causes of Corrosion in High Pressure Boiler Tubes. (Blast Furnace and Steel Plant, 1947, vol. 35, May, pp. 578, 580). A review is given of a paper by P. M. Brister and J. B. Romer, presented to the (U.S.A.) Electro-Chemical Society, in which an analysis was made of the causes of corrosion of metals used for steam generating tubes and superheater tubes in high-pressure boilers.—J. R.

Steam Turbine Blade Deposits. F. G. Straub. (Illinois University, 1946, Engineering Experiment Station Bulletin Series No. 364). Studies to determine the causes of steam turbine blade deposits are reported. Two types of deposits were encountered—water-soluble and insoluble.—R. A. R.

Corrosion and Oxidation Experiences in High Pressure and High Temperature Steam Service. P. M. Brister and J. B. Romer. (Electrochemical Society, 1947, Preprint No. 91-17). The causes and prevention of the corrosion of steam-generating and superheater tubes for high-pressure boilers are discussed. It is pointed out that with the proper selection of ferritic chromium–molybdenum steels, the corrosion by steam and flue gases should not be troublesome for metal temperatures up to about 650° C., stainless steels being satisfactory even at somewhat higher temperatures.—D. R. S.

High Temperature Corrosion of Metals under Alternate Carburization and Oxidation. H. K. Ihrig. Electrochemical Society, 1947, Preprint No. 91-23). The corrosion of a number of alloy steels under alternating oxidizing and carburizing conditions at high temperature was studied with a view to their application in apparatus for certain industrial catalytic processes. The results lead to the conclusion that high-chromium steels, *e.g.*, 26%-chromium steel, are best for use under such conditions.—D. R. S.

The Oxidation of Metals. W. E. Campbell and U. B. Thomas. (Electrochemical Society, 1947, Preprint No. 91-24). A simple method for measuring low oxidation rates at elevated temperatures is described; it is based on the measurement of the drop in pressure in a constant volume caused by the consumption of oxygen by the metal. The method was applied to stainless steel and a number of non-ferrous metals and alloys at temperatures of 194°, 256°, and 302° C. Only stainless steel was found to conform accurately to the logarithmic law. An explanation why the other metals and alloys studied do not follow this law is offered.—D. R. S.

The Internal Corrosion of Cans. D. Dickinson. (University of Bristol: Annual Report of the Fruit and Vegetable Preservation Research Station, Campden, 1945, pp. 32–39). In this, the third Progress Report on an investigation of the internal corrosion of tinplate cans containing fruits, the author outlines a theory to account for the corrosion of cans made from both hot- and cold-rolled steel. The results of tests of the "corrosivity" of different varieties of plums are also presented.—C. O.

Acetic Acid and Chlorine *vs.* Materials of Chemical Plant Construction—Stainless Steel. (Chemical Engineering, 1947, vol. 54, May, pp. 241, 246). The resistance of chromium–nickel and chromium–nickel–molybdenum stainless steels to attack by acetic acid, chlorine, and chemicals used for bleaching textiles is discussed.—R. A. R.

Contact Corrosion Problems in the Metal Window Industry. E. F. Pellowe and F. F. Pollak. (Metallurgia, 1947, vol. 36, June, pp. 67–70). The corrosion of cast aluminium in contact with steel, galvanized steel, brass, pure aluminium, and stainless steel has been studied by determining the loss of weight of pairs of discs after alternate immersion in salt solution and tap water, and drying for a period of two months. The results for unpainted samples were of the same order as those obtained by the American Society for Testing Materials. Corrosion was negligible in the samples which were painted.—C. O.

ECONOMICS AND STATISTICS

The Dislocation in the World Production of Iron Caused by the War. R. Durrer. (Von Roll Werkzeitung, 1947, vol. 18, Mar., pp. 29–30). Statistics on the production of iron and steel by the principal industrial countries of the world from 1870 to 1945 are presented and some marked changes caused by the war of 1939–1945 are pointed out.—R. A. R.

Reconstruction of the British Steel Industry. (Iron and Coal Trades Review, 1947, vol. 154, June 20, pp. 1115–1117). Plans and statistics relating to the reconstruction of the British steel industry are outlined and their relation to the general world shortage of steel is discussed.—R. A. R.

Iron and Steel Developments. (Iron and Steel, 1947, vol. 20, May 23, pp. 203–204). The duties of the Steel Board are outlined, and the first six months of its work are reviewed. The modernization and development plan put forward by the British Iron and Steel Federation has been accepted by the Board, and some details of the plan are discussed.—J. R.

The Iron and Steel Industry of Canada. (British Iron and Steel Federation, Monthly Statistical Bulletin, 1947, vol. 22, Apr., pp. 1–10). The development of the Canadian iron and steel industry is traced since the first blast-furnace was built in 1737. Notable expansion of the industry occurred under the stimulus of each of the two world wars, and the present annual capacity is over 3 million tons of steel. Use of native ores is increasing, but much of the ore, limestone, and coking coal is still imported. The present tariff position is summarized. Pre-war and post-war capacities, imports, and exports are compared, and it is considered that there will continue to be a market for imported steel

products, particularly on the West Coast, of which the United Kingdom may well take advantage.—M. A. V.

The Status of the Steel Industry at the Close of 1946. W. S. Tower. (Blast Furnace and Steel Plant, 1947, vol. 35, Jan., pp. 55–56). A survey is made of the output, capacity, and economics of the American iron and steel industry during 1946. Although some 14,000,000 tons of finished-steel production were lost owing to strikes and stoppages within and without the industry, output reached a peace-time record of 65,800,000 ingot tons. —J. R.

Supply of Scrap for 1947 Largely Dependent upon Uninterrupted Production in Generating Industries. E. C. Barringer. (Blast Furnace and Steel Plant, 1947, vol. 35, Jan., pp. 57–59). The prospects of obtaining the scrap required for American steelworks and iron foundries during 1947, and the future prospects are reviewed. —J. R.

Iron and Steel in the Philadelphia District. T. J. Ess. (Iron and Steel Engineer, 1947, vol. 24, May, pp. s-19–s-30). The history of the iron and steel industry in the region within a radius of 150 miles of Philadelphia is outlined. Statistics show the distribution of blast-furnace, steel-melting, and rolling-mill capacity in the district, which today contains between 11% and 12% of the total ingot capacity of the United States. Shortage of Lake Superior iron ores, necessitating the importation of foreign ores, may restore to Philadelphia the pre-eminence it attained at the end of the eighteenth century. —C. O.

Swedish Mining Statistics, Collected from Chambers of Commerce Reports. (Jernkontorets Annaler, 1947, vol. 131, No. 2, pp. 63–74). [In Swedish]. Eighteen tables are presented giving details, for the period 1911–1945, of the production of Swedish ores, concentrates, sinter, pig iron, ferro-alloy, steel ingots, and semi-finished iron and steel.—R. A. R.

The Production of the German Iron and Steel Industry from 1938 to 1944. H. Schmitz. (Stahl und Eisen, 1947, vol. 66–67, Jan. 30, pp. 48–56). Detailed statistics on the production of iron, steel, ferro-alloys, castings, and rolled products in Germany from 1938 to 1944 are given.—R. A. R.

Present Position of the German Steel Industry. H. Schmitz. (British Iron and Steel Federation, 1947, vol. 22, Mar., Bulletin No. 3, pp. 1–7). This is an English version of the paper published in Stahl und Eisen, 1947, vol. 66–67, Jan. 30, pp. 48–56 (*see* preceeding abstract).

War Effects on the German Steel Industry. J. E. Graf. (Iron and Steel Engineer, 1947, vol. 24, Feb., pp. 53–57). —R. A. R.

The Bottle Neck "Labour Force" and Its Importance in the German Iron and Steel Industry. H. Euler. (Stahl und Eisen, 1947, vol. 66–67, Mar. 27, pp. 103–112).

Italian Iron and Steel. A. Giordano. (Iron and Steel, 1947, vol. 20, May 23, pp. 205, 280). Production data for the Italian iron and steel industry for the first nine months of 1946 are given, and the future possibilities of the industry are discussed.—J. R.

The Italian Pig-Iron and Steel Industry. H. J. Becker. (Foundry Trade Journal, 1947, vol. 82, June 12, pp. 137–138). Statistics are given of the Italian iron and steel industry, which has made some progress towards reaching its pre-war level despite the difficulties resulting from war damage and shortages of power and raw materials.—J. R.

Italian Pig-Iron and Steel Industry. (Iron and Coal Coal Trades Review, 1947, vol. 154, June 13, pp. 1085–1087). Statistics are given on the size and organization of the Italian iron and steel industry before the war, the damage suffered during the war, and the present state of the industry.—C. O.

Statistics on Production and Consumption of Pig Iron, Steel Ingots, and Finished Steel in Japan and Korea. (British Intelligence Objectives Sub-Committee, 1947, Report No. B.I.O.S./J.A.P./P.R./81: H.M. Stationery Office).—C. O.

Demonstrating the Validity of Economic Laws. K. Daeves and A. Beckel. (Stahl und Eisen, 1947, vol. 66–67, Mar. 27, pp. 112–116). Graphical methods of presenting and comparing production and other economic statistics are described. The Gauss law is explained with an example of its application to predict the market saturation point.—R. A. R.

Relationships between Heat and Power Economy and Production Conditions in the Larger Iron and Steel Works. F. Wesemann. (Stahl und Eisen, 1947, vol. 66–67, Jan. 30, pp. 35–42). A quantitative analysis is made of the statistics relating to the factors affecting the consumption of heat per ton of steel ingots in order to develop an efficiency index which enables the performance of different works to be compared. This statistical technique also provides a means of rapidly detecting the weak points in the thermal economy of a works. —R. A. R.

Plant Evaluation. K. Rummel. (Stahl und Eisen, 1947, vol. 66–67, Apr. 24, pp. 153–159). Equations are derived for calculating the value of plant taking into account the expected life, interest on capital, operating costs, and the proceeds of sales. Examples of their application are given.—R. A. R.

An Economic Approach to Hardenability. D. I. Brown. (Iron Age, 1947, vol. 159, Mar. 27, pp. 42–47; Apr. 3, pp. 53–55, 160). The possibilities of quoting prices for steel on the basis of hardenability are considered —R. A. R.

MISCELLANEOUS

The Scheduling of Maintenance Shops. L. E. Fuller. (Iron and Steel Engineer, 1947, vol. 24, May. pp. 35–41). The organization of the allocation of the work and materials to the various departments of an industrial concern is discussed at length. Examples of the types of schedule cards used in the planning departments of a steelworks are illustrated.—C. O.

Electronics as an Aid to Production. H. A. Thomas. (Board of Trade Journal, 1947, vol. 153, July 26, pp. 1266–1267). The industrial applications and possibilities of electronic devices, *e.g.*, control, counting, testing, and inspection, are discussed.—M. A. V.

Plant Freezer for Fitting Huge Eccentric Shafts. (Steel, 1947, vol. 120, Feb. 10, pp. 98–99, 108). A short description is given of a technique used for fitting roller bearings on to large eccentric shafts. In addition to heating the bearing to 200° F., the shafts were cooled in dry ice for 24 hr. to about —100° F. in order to obtain the necessary clearance.—C. O.

Pipe-Line Insulation Minimizes Steam Losses. (Steel, 1947, vol. 120, Feb. 17, pp. 107–110). A description is given of a pipeline, 14 in. in dia., constructed to carry superheated steam, above ground, for a distance of 3000 ft. The pipe was insulated with 2½ in. of mineral wool, backed by metal laths, and protected by asphalt-impregnated asbestos felt. The temperature drop between the ends when carrying steam at 590° F. was only 10–15° F., and the line operated for two years without any deterioration.—C. O.

Current Trends in the Re-Use of Water. C. F. Hauck. (Engineers' Society of Western Pennsylvania: Blast Furnace and Steel Plant, 1947, vol. 35, May, pp. 543–548). The conservation of water supplies by (*a*) the

re-use of process waters, (*b*) the re-use of water from cooling and heating circuits, and (*c*) the re-use of sewage-plant effluent, are discussed, with particular reference to steelworks requirements.—J. R.

Aluminium Therapy and Silicosis Prevention. I—Medical Aspects. W. D. Robson. (Transactions of the Canadian Institute of Mining and Metallurgy, 1947, vol. 50, pp. 57–67). The prevention and treatment of silicosis by inhaling air carrying particles of aluminium has already been reported (*see* Journ. I. and S.I., 1945, No. I, p. 97A). In this paper medical aspects of the subject and proper procedure in applying aluminium therapy are dealt with.—R. A. R.

Aluminium Therapy and Silicosis Prevention. II—Engineering Aspects. A. W. Jacob. (Transactions of the Canadian Institute of Mining and Metallurgy, 1947, vol. 50, pp. 68–83). The equipment for applying aluminium therapy (*see* preceding abstract), and methods of controlling the purity and particle size of the aluminium powder used are described.—R. A. R.

Smoke and Dust Control, Heating and Ventilation in the Sorel Plant. (Industrial Heating, 1947, vol. 14, Feb., pp. 236–242; Mar., pp. 422–432). The equipment used for heating the atmosphere, the dust and smoke control system, and the ventilation, at an ordnance factory at Sorel, Quebec, are described and illustrated. —C. O.

BOOK NOTICES

Beaumont, R. A. "*Metallurgy for Aircraft Engineers, Inspectors and Engineering Students.*" 8vo, pp. viii + 269. Illustrated. London, 1946: Sir Isaac Pitman and Sons, Ltd. (Price 25*s*.).

This book contains the following chapters: The Production of Steel; Mechanical Methods of Working Steel; Defective Materials and Processes; Steel—Composition, Structure and Heat-Treatment; Alloy Structural Steels; Case-Hardening Steels; Light Alloys; Copper and Its Alloys; Essentials of Mechanical Testing; and Temperature-Measuring Equipment.—R. E.

Broken Hill Proprietary Co., Ltd. "*Staff Training Handbook.*" Sm. 8vo, pp. 133. Newcastle, N.S.W., 1947.

In this, the first post-war edition of the "Staff Training Handbook" of the Broken Hill Proprietary Co., Ltd., and its Associated Companies, the history and development of the training scheme is traced from its commencement over twenty years ago. The scheme, which is described, affords employment to cadets, trainees and apprentices, both technical and commercial, who are given opportunities for wider experience and are encouraged to prepare themselves for promotion, in order that the higher administrative and executive positions may be filled from within the organization.—R. E.

Cady, W. G. "*Piezoelectricity.*" An Introduction to the Theory and Applications of Electromechanical Phenomena in Crystals. First Edition. 8vo., pp. xxiii + 806. Illustrated. New York and London, 1946: McGraw-Hill Book Co., Inc. (Price 45*s*.).

The author presents a comprehensive treatise on the entire field of piezoelectricity, including the related fields of elasticity, dielectrics, optics and magnetism. Beginning with fundamental principles, the book covers all aspects of piezoelectric theory and its principal applications. A unified account is given of experimental results, with many formulæ, numerical data, and an extensive bibliography.—R. E.

Dunkley, G. T. "*Controlled Atmospheres in Heat Treatment.*" 8vo, pp. 69. Illustrated. London, 1947: Sir Isaac Pitman and Sons, Ltd. (Price 7*s*. 6*d*.).

This is a review of the uses of controlled atmospheres in industry at the present time. The methods of production and their uses in the metal industries are described, and the elementary theory of the gas-metal reactions which occur is discussed.—R. E.

Gaffert, G. A. "*Steam Power Stations.*" Third Edition. 8vo, pp. xii + 604. Illustrated. New York and London, 1946: McGraw-Hill Book Co., Inc. (Price 27*s*. 6*d*.).

This book covers steam power-plant from the standpoint of construction of various types of equipment, their performance characteristics, economics, and integration in the complete plant.—R. E.

Gregory, E. and E. N. Simons. "*Stainless and Heat-Resisting Steels Simply Explained.*" 8vo, pp. 131. Illustrated. London, 1947: Hutchinson and Co. (Publishers), Ltd. (Price 8*s*. 6*d*.).

There can be little doubt that a demand exists for a work of this kind. The systematic literature on stainless and heat-resisting steels is not yet voluminous and most of it is for the metallurgical specialist. Thus the engineer or designer who seeks to make the best use of modern materials will welcome this volume.

The authors have faced their self-imposed task in a commendable way by devoting the greater part of their work to the manipulation, fabrication and treatment of stainless steels, metallurgical theory being accorded a secondary place. While this unequal sub-division is rightly conceived, it could have been implemented better by the omission of Chapter 2, for surely it is beyond the requirements of the potential user of these special steels to learn anything about the methods of manufacture, let alone the calculation of a furnace charge.

The first chapter surveys modern knowledge of corrosion and creep, so that the reader is at this early stage made aware of the conditions that stainless and high-temperature steels have to withstand. Chapter 2 deals shortly with methods of manufacture, and the next one classifies the principal types of steel and describes their salient properties. The next seven chapters cover in some detail all the processing operations to which stainless steels may be subjected, and it is this section of the book that the reader will find most informative. The techniques of machining, welding, soldering and brazing, heat-treatment, hot and cold working, and pickling and polishing are all covered in separate chapters and are likely to provide the answer to many of the problems confronting the user of these steels. Finally, there are four chapters on stainless "clad" steels, castings, testing and inspection, and sundry applications.

The authors have fulfilled their task, on the whole, satisfactorily, but the book gives the impression of having been written and arranged rather hurriedly. All the framework of a systematic treatment of the subject is there but it has not always been used to the best advantage. The text is often out of step with the sub-headings and the general arrangement could be improved. The lists of reagents, coupled with their

effect on various qualities of stainless steel, contain some surprising and unnecessary items, while others can hardly be dealt with so summarily as is done here. As this information is likely to be frequently referred to, the authors' natural desire for brevity might lead to misconception and disappointment. One outstanding example of insufficient care in finishing the text is on the lower half of page 32 *et seq.* The obvious misstatements and general confusion of facts here call for immediate deletion.

The volume is well produced, and includes a number of good plates which, however, do little to assist the text. It falls neatly into place in the literature of the subject and it is unfortunate that minor defects intrude to mar its value, but the core of the work is sound and many will have cause to thank the authors for this simplification of a complicated subject..

STEPHEN L. ROBERTON.

GOURLEY, J. C. "*Welding Symbols.*" 8vo, pp. vii + 115. Illustrated. Milwaukee, 1947 : The Bruce Publishing Co. (Price 13*s.* 6*d.*).

The standards and symbols explained in this book are those recognised by the American Welding Society. The general standards for the use of arc- and gas-welding symbols, as well as resistance-welding symbols are explained in simple language.—R. E.

GRISWOLD, J. "*Fuels, Combustion and Furnaces.*" First Edition. (Chemical Engineering Series). 8vo, pp. vii + 496. Illustrated. New York and London, 1946 : McGraw-Hill Book Co., Inc. (Price 27*s.* 6*d.*).

This book covers the fundamental background and the more essential specific details of the technology, combustion properties, and utilization of fuels, as well as the apparatus and equipment involved. The book also introduces the student to petroleum refining technology and engineering, chemical engineering, thermodynamics, chemical kinetics, the technology and utilization of refractories, and the process industries that use furnaces and heaters.—R. E.

"HEAT-TREATER." "*Fundamental Principles and Applications of Induction Heating.*" 8vo, pp. 147. Illustrated. London, 1947 : Chapman and Hall, Ltd. (Price 10*s.* 6*d.*).

This book discusses the development and applications of industrial heating, the principles of induction and coil design, the choice of equipment producing different frequencies, hardening technique and the employment of induction heating, internal hardening and assembly processes, as well as miscellaneous applications.—R. E.

JOHNSON, F. "*Metal Working and Heat Treatment Manual.*" Volume II. "*Alloy Steels, Cast Iron and Non-Ferrous Metals.*" 8vo, pp. 226. Illustrated. London : Paul Elek Publishers, Ltd. (Price 17*s.* 6*d.*).

This is the second of a series of four volumes covering the entire range of the heat-treatment of metals. It is written primarily for the engineer and the student of engineering, but metallurgists will also find it useful and informative.—R. E.

KUNITZ, H. A. "*Oil Heating Handbook.*" Third Edition, Revised. 8vo, pp. x + 464. Illustrated. Philadelphia and London, 1947 : J. B. Lippincott Co. (Price 30*s.*).

After commencing with a short introductory chapter and a description of a typical oil-burner installation, the remainder of the book is in four parts. Part II deals with fuels and combustion. Part III is concerned specifically with heating, including operative controls. Part IV takes up the installation and operation of oil burners, whilst retail sales are considered in Part V.

Informative charts and tables are placed within the chapters to which they naturally belong, or into chapter XX, which harbours information of a more general character. The rather voluminous glossary is placed at the end.—R. E.

SIMONDS, H. R. and A. BREGMAN. "*Finishing Metal Products.*" Second Edition. 8vo, pp. xii + 352. Illustrated. New York and London, 1946 : McGraw-Hill Book Co., Inc. (Price 20*s.*).

This new edition describes many developments in the metal-finishing field which took place during the war and during the years immediately preceding it. It brings up-to-date the subject as covered in the first edition and includes additional material. A chapter on metal colouring is included.—R. E.

SMART, R. C. "*The Technology of Industrial Fire and Explosion Hazards.*" 8vo. Vol. 1, pp. 202. Vol. 2, pp. 184. London, 1947 : Chapman and Hall, Ltd. (Price 16*s.* per vol.).

The author of these books is consulting engineer and surveyor to the Fire Offices, Board of Trade, and other Government Departments, and has had first-hand experience over many years in investigating the causes of industrial fires and their prevention. His work summarizes and explains the origin of fires and explosions in industrial premises, and by giving a detailed scientific analysis of the circumstances leading to the existence of the hazards, he provides a valuable educational service to industry.

Apart from the first two, all the chapters deal with the fire and explosion risks of specified industries or processes and work through agriculture, fuels, gases, heat-treatment, light alloys, wood, solvents, waxes, paints, explosives, plastics and many other special cases. The author has drawn freely from Factory Department reports and other official documents to illustrate his instruction by actual example, and the effectiveness of his work is thereby enhanced. Generally speaking the main purpose of the book is to draw attention to the existence of the fire danger and in this it succeeds beyond question. Preventative action is indicated in a great many instances, but it is given only incidentally to the main theme, and the control and extinguishing of fires once started are dealt with to a lesser degree.

Works managers, safety officers and committees will find much to interest them in the chapters dealing with their particular industry, and it is of great help to have the author's wide experience concentrated in book form. The volumes are well produced, the arrangement is logical and convenient and the only defects are minor slips attributable to the proof reader rather than to the author.

STEPHEN L. ROBERTON.

SMITH, P. I. "*Materials of To-morrow.*" Second Edition. 8vo, pp. 156. Illustrated. London, New York, Melbourne and Sydney, 1946 : Hutchinson's Scientific and Technical Publications. (Price 12*s.* 6*d.*).

An interesting account is given of the new materials of commerce including plastics, synthetic rubber, light metals, new steels, new woods, synthetic fibres, glass, concrete, ceramics, synthetic adhesives and new drugs. This edition has been revised and rewritten. —R. E.

TOLANSKY, S. "*High Resolution Spectroscopy.*" 8vo, pp. xi + 291. Illustrated. London, 1947 : Methuen and Co. Ltd. (Price 21*s.*).

This book discusses both the theory and the practice of the refined experimental optical techniques of high-resolution spectroscopy, including treatment of

light sources, interferometers, microphotometry, etc. —R. E.

WREDDEN, J. H. "*The Microscope—Its Theory and Applications.*" With an Historical Introduction by W. E. Watson-Baker. 8vo, pp. xxiv + 296. Illustrated. London, 1947 : J. and A. Churchill, Ltd. (Price 21*s.*).

This book has been designed to meet the needs of students in various branches of science and industry. It explains simply the optical principles, construction and components of the microscope. The development of the present-day microscope is dealt with in an historical introduction contributed by W. E. Watson-Baker. Details are also given regarding the selection of specific types of microscopes and their applications including photomicrography. The preparation of specimens is also dealt with.—R. E.

NEW PUBLICATIONS

BENNETT, H. "*Concise Chemical and Technical Dictionary.*" 8vo, pp. xxx + 1055. New York City, 1947 : Chemical Publishing Co. (Price $10).

BUTLER, G. M. "*A Pocket Handbook of Minerals.*" 8vo, pp. 311. Illustrated. Gardenvale, Quebec : Canadian Mining Journal. (Price $4.25).

CRAMER, H. "*Mathematical Methods of Statistics.*" (Princeton Mathematical Series 8). 8vo, pp. xvi + 575. Princeton, N.J., 1946 : Princeton University Press ; London : Oxford University Press. (Price 33*s.* 6*d.*).

DAVIDENKOV, N. N. "*Some Problems in the Mechanics of Materials.*" [In Russian.] Pp. 152. Illustrated. Leningrad, 1943 : Leningradskoe Gazetno-Zhurnal'noe u Knizhnoe Izdatel'stvo. (Price 8 roubles).

DAVIDENKOV, N. N. "*The Problem of Impact in Metallurgy.*" [In Russian.] With an Appendix : "*Statistical Methods of Treating the Results of Impact Tests.*" By N. P. Shchapov and A. I. Kochetov. 8vo, pp. 116. Illustrated. Moscow and Leningrad, 1938 : Izdatel'stvo Akademii Nauk, S.S.S.R.

DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH. Fuel Research. Technical Paper No. 49. "*The Characteristics of Pulverised Coal. Effect of Type of Mill and Kind of Coal.*" London, 1947 : H.M. Stationery Office. (Price 1*s.* 3*d.*).

DUNKLEY G. T. "*Controlled Atmospheres in Heat Treatment. Their Methods of Production and Uses in The Metal Industries.*" London : Sir Isaac Pitman and Sons, Ltd. (Price 7*s.* 6*d.*).

"*Fundamental Principles and Applications of Induction Heating.*" By "HEAT-TREATER." Pp. 147., London : Chapman and Hall, Ltd. (Price 10*s.* 6*d.*)

GAFFERT, G. A. "*Steam Power Stations.*" Third Edition. 8vo, pp. 613. Illustrated. London : McGraw-Hill Publishing Co., Ltd. (Price 27*s.* 6*d.*).

GALASSINI, A. "*Leghe metallische e siderurgia.*" Sixth Edition. 8vo, pp. xii + 328. Illustrated. Milan, 1945 : Ulrico Hoepli. (Price L.420).

GALASSINI, A. "*Lavorazione a caldo. Trattmaenti termici. Prove di Materiali.*" 8vo, pp. xvi + 392. Illustrated. Milan, 1946 : Ulrico Hoelpli. (Price L.500).

GAVIOLI, G. "*Metodi di analisi chimica siderurgica.*" 4to, pp. xix + 378. Illustrated. Milan, 1947 : Ulrico Hoepi. (Price L.200).

GOURLEY, V. C. "*Welding Symbols.*" Milwaukee : Bruce Publishing Co. (Price $2.50).

HAUSNER, H. H. "*Powder Metallurgy.*" Brooklyn, N.Y. Chemical Publishing Co., Inc. (Price $7.00).

HENDRY, J. W. "*A Manual of Time and Motion Study.*" Second Edition. London : Sir Isaac Pitman and Sons, Ltd. (Price 12*s.* 6*d.*).

HOLLOMON, J. W. and L. D. JAFFE. "*Ferrous Metallurgical Design.*" (Design Principles for Fully Hardened Steel). 8vo, pp. 140. Illustrated. New York City : John Wiley and Sons. (Price $5).

HUGHES, T. P. "*Production and Processing of Metals and Plastics.*" Chicago, 1947 : Irwin-Farnham Publishing Co.

JOHNSON, C. G. "*Metallurgy.*" Third Edition. 8vo, pp. 418. Chicago, 1946 : American Technical Society. (Price $5.00).

KLOEFFLER, R. G. "*Principles of Electronics.*" 8vo, pp. 175. London : Chapman and Hall, Ltd. (Price 16*s.* 6*d.*).

KURREIN, M. and F. C. LEA. "*Cutting Tools for Metal Machining.*" Second Edition. 8vo, pp. 312. London, 1947 : Charles Griffin and Co., Ltd. (Price 28*s.* 6*d.*)

LI, K. C. and CHUNG YU WANG. "*Tungsten.*" Second Edition. New York : Rheinhold Publishing Corporation. (Price $8.50).

MACCHIA, O. "*Le Chromage Electrolytique.*" 8vo, pp. 444. Illustrated. Paris, 1940 : Librairie Dunod.

MELLER, C. "*Manuel de la soudure à l'arc.*" Traduit de l'allemand par P. Neumayer. 2e édition. 8vo, pp. 202. Illustrated. Paris : Dunod (Price 320 fr.).

MICHALIK, A. and L. EBERMAN. "*Spanabhebende Metallbearbeitung. Einführung in die Bearbeitung mit Werkzeugmaschinen.*" 2 Auflage. pp. iv + 222. Illustrated. Zürich, 1945 : Schweizer Druck- und Verlagshaus. (Price 8·50 Swiss francs).

MILLER, J. L. "*Modern Assembly Processes—Their Development and Control.*" Second Edition. London : Chapman and Hall, Ltd. (Price 18*s.*).

MORUCCI, R. "*Trattamenti termici dei metalli.*" Pp. viii + 252. Illustrated. Milan, 1946 : Ulrico Hoepli. (Price L.350).

"*Radiography in Modern Industry.*" 8vo, pp. 122. Rochester, N.Y. : Eastman Kodak Co. (Price $3).

"*Resistance Welding Manual.*" Revised Edition. 8vo, pp. 552. Illustrated. Philadelphia, Pa. : Resistance Welder Manufacturers' Association. (Price $3).

SMITH, O. C. "*Identification and Qualitative Chemical Analysis of Minerals.*" 8vo, pp. 351. New York City, 1946 : D. Van Nostrand Co. (Price $6.50).

SOCIETY OF AUTOMOTIVE ENGINEERS. "*Boron Steel Report.*" La. 8vo, pp. 72. New York : The Society. (Price $2·00).

"*Thorpe's Dictionary of Applied Chemistry.*" Fourth Edition. Vol. VIII. "*Meth-Oils, Essential.*" London : Longmans Green and Co., Ltd. (Price £4).

TIBBENHAM, L. J. "*Oxy-Acetylene Welding Handbook.*" Second Edition. Stowmarket : Suffolk Iron Foundry (1920), Ltd.

VALEUR, J. P. LAURENT and S. BOGROFF. "*Les bases de la résistance mécanique des métaux et alliages.*" 8vo, pp. 290. Illustrated. Paris : Librairie Dunod. (Price 1200 francs).

WABER, J. T. and H. J. MCDONALD. "*Stress Corrosion Cracking of Mild Steels.*" Pp. 90. Pittsburgh : Corrosion Publishing Co. (Price $2).

WREDDEN, J. H. "*The Microscope, Its Theory and Applications.*" 8vo, pp. 320. Illustrated. London, 1947 : J and A. Churchill, Ltd. (Price 21*s.*).